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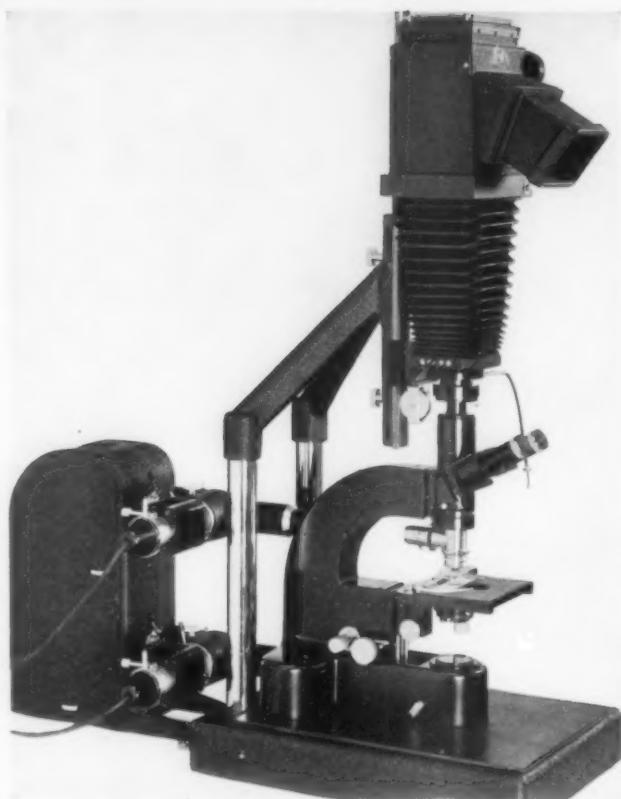
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The authors employ the methods of physics, chemistry and biochemistry to explore the ultrastructure of the cell and to interpret the nature of intracellular processes and the functional sig-

nificance of cellular structure. The text opens with an introduction to the organization of living matter. The structure, morphological and cytochemical organization of the cell are fully described. Submicroscopic organization is discussed, and interesting accounts given of the significance of cytoplasmic organoids. Other important topics are: molecular structure of plasma membrane, cell permeability, chromosomes and cell division, cellular activity, composition of the nucleus, and death of the cell.

Many references are given at the end of each chapter. Among the illustrations are some of the finest electron micrographs ever published.

By E. D. P. DEROBERTIS, M.D., Head of the Department of Cell Ultrastructure, Institute for the Investigation of Biological Sciences, Montevideo; W. W. NOWINSKI, Ph.D., Associate Professor of Biochemistry, University of Texas Medical School, Galveston; and FRANCISCO A. SAEZ, Ph.D., Head of the Department of Cytogenetics, Institute for the Investigation of Biological Sciences, Montevideo. 456 pages, 6¼" x 9¼", with 162 illustrations. \$7.75. *Second Edition!*

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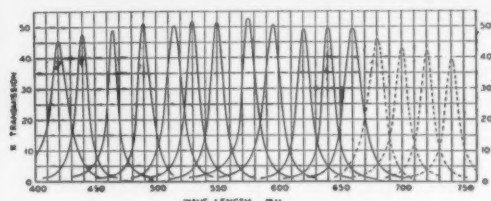
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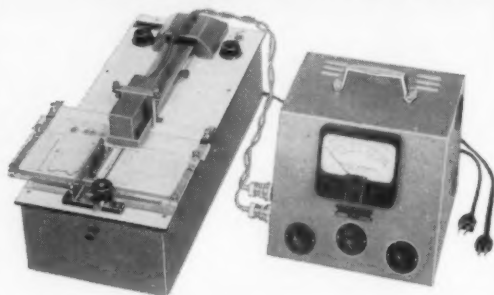
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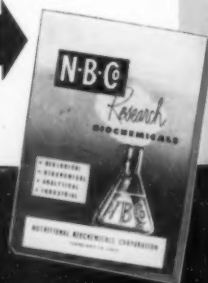
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H. J. Muller and the Geneva Conference

The Atomic Energy Commission was technically well within its rights in barring H. J. Muller's paper that was prepared for presentation at the Geneva conference (see page 822). It was understood from the beginning that not all papers submitted could be accepted. And there was certainly no obligation to name Muller as an official delegate. Nevertheless, many persons will regret the affair, because devious methods appear to have been used to keep Muller off the program and because his viewpoint, which happens to differ significantly from that of the commission, was apparently not as fully represented at the conference as most geneticists would have wished.

If one adopts the generous view that the AEC officials acted in good faith and that the final outcome was the simple result of ineptness, there still remains the question of whether the decision to bar Muller's paper was a wise one. Many of those who know Muller's international reputation as a competent and responsible expert on the genetic effects of high-energy radiation will believe not. The scientists who attended the panel meeting at Geneva on "Genetic effects of radiation: human implications" expressed themselves wordlessly but eloquently by giving Muller a standing ovation as he sat silent in the audience. No doubt, their reaction is shared by large numbers of scientists in this country—inside the AEC as well as outside.

Although there is widespread agreement among geneticists regarding the qualitative effects of high-energy radiation on hereditary material, much remains to be learned about the quantitative effects, particularly at low dosages and on man. It is also true that there are wide gaps in our knowledge of the direct effects of very low dosages of radiation on man. At a time when large human populations are being exposed to small amounts of radiation in addition to natural background, information on these points becomes extremely important. It makes no difference whether the radiation comes from weapons of war or from the many peacetime uses of atomic energy that can now be foreseen. Research workers in AEC-financed national laboratories and in many other laboratories are trying their best to get this information as quickly as possible. AEC is doing a magnificent job of helping them—through financial support and otherwise.

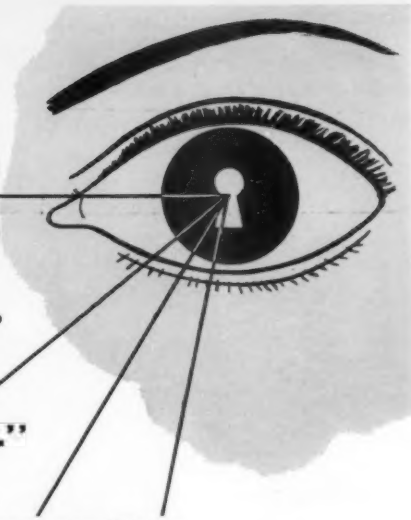
It is tremendously important at this time that there be free and open discussion of all possible radiation dangers. Only in this way can maximum progress be made in evaluating the dangers and taking necessary steps to reduce or eliminate them. There is no magic formula for arriving at a figure for the so-called "permissible dose." In Handbook 59 of the U.S. National Bureau of Standards, in which this term is defined and its origin reviewed, it is made clear that this dosage—0.3 roentgen units of x-rays per week for whole-body irradiation—should be permitted only for adults exposed in small numbers. For children or large populations, it is recommended that the usual value be divided by 10. With the prospects of a great increase in peacetime use of atomic energy, we cannot too soon know what are reasonable upper limits of radiation where large numbers of persons are involved. Important precedents are being established, and it will become increasingly costly and difficult to modify them if they should prove to be inadequate.

There are therefore compelling practical reasons, to be added to the obvious ones that should apply at all times to all forms of knowledge, for resisting any authoritarian or arbitrary suppression of free and open discussion of the hazards to man of radiation.—GEORGE W. BEADLE

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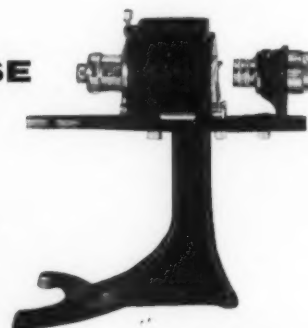


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Another Look at the Ice Age

William Lee Stokes

The recognition of widespread glaciation as an explanation for numerous details of topography, geology, and biological distribution ranks with the greatest achievements of scientific observation and reasoning. The underlying cause of glaciation, however, remains in doubt. Textbooks on geology and climatology usually devote a page or two to a listing of the various theories, with emphasis on the ones currently most popular, but it is significant that two of the latest textbooks on historical geology make no mention at all of the probable causes of ice ages. This may reflect a cautious attitude on the part of textbook writers and a lack of agreement among the experts, but it may also demonstrate a lag in interest or a feeling that the problem is hopeless of solution.

Certainly the subject is not diminishing in importance, for it has ramifications extending into the realms of astronomy, climatology, geology, and biology that were unappreciated a few years ago. The evidence of gradually changing climate and rise of sea level has become of increasing concern to business and military minds. Perhaps the problem of glaciation, like so many other interesting and vital subjects when they become dismembered among different departments of learning, is being avoided because no one feels justified to attempt a synthesis for fear of showing ignorance in related fields that are also involved.

At least 29 "explanations" have been advanced to account for widespread glaciations. Most of these had little chance of survival from the first, but others enjoyed some degree of success until they

were rendered untenable by subsequently accumulated information. It is not the purpose of this article (1) to list or discuss these theories. They have been adequately treated in several widely available books (2, 3).

Ideal Theory

An ideal theory of glaciation should satisfy as simply as possible three chief requirements: (i) an initiating event or condition, (ii) a mechanism for cyclic repetitions or oscillations within the general period of glaciation, and (iii) a terminating condition or event. It is agreed by most students of the subject that the first requirement is met by mountain building. Elevation of large tracts of the earth is the one invariable prerequisite of the major glaciations proved by geology. Much more difficult is the problem of the great oscillations of climate, seemingly independent of topographic change, which occur within the glacial interval. The termination of glaciation apparently awaits only the lowering of land areas by erosion or crustal adjustments so that rainfall replaces snowfall as the chief form of precipitation.

Although extraordinary or even catastrophic events may have caused the ice ages and their oscillations, it is nevertheless true that the ideal theory ought to fit within the framework of uniformitarian principles. A theory that relies on unprovable, unobservable, or unpredictable conditions, when well-known or more simple ones will suffice, cannot be widely accepted. Among the more or less spectacular hypothetical events that have been invoked as possible causes of the ice ages are flooding of the ocean by sub-

marine lava, radiations from a warm moon, rapid oscillations of the continents in and out of stratospheric elevations, and passage of the earth through great clouds of cosmic dust. Since none of these have been observed, they must remain merely hypothetical possibilities.

Less open to question but still lacking adequate scientific demonstration are such ideas as long-term variations in solar energy output, tilting of the earth's axis, wandering of the poles, variations in radioactive warming of the earth, and radical changes in distribution of continents and ocean basins.

Any theory will be greatly strengthened if quantitative data can be brought to its support. Of course, past events are difficult to analyze mathematically; but, if measurements of existing climatic phenomena can be shown to have a bearing on past glacial periods, the basis of proof will be greatly broadened.

The ideal theory must be prepared to explain simultaneous glaciations over the entire earth. Evidence is now conclusive that the glaciations on opposite sides of the Atlantic were concurrent events. Separate or alternating glaciations of the Northern and Southern hemispheres, once thought to be possible, evidently did not occur in the Quaternary period.

Last but not least, the theory must explain the greatest paradox of all—the evidence of cold and ice existing and increasing simultaneously with conditions that favored accelerated evaporation and precipitation. In other words, lowering temperatures and greater precipitation are considered to have existed side by side on a world-wide scale and over a long period in apparent defiance of sound climatological theory. Among the many quotations that could be cited reflecting the need for a more comprehensive explanation of this difficulty the following seems typical (4).

"In the Arequipa region [of Peru], as in many others in both hemispheres where Pleistocene conditions have been studied, this period appears to have been characterized by increased precipitation as well as lowered temperature. If, however, precipitation then was greater over certain large areas of the earth's surface than it is at present, a corollary seems to be implied that over other large areas evaporation was greater than normal to supply increased precipitation, and hence in these latter areas the climate was

Dr. Stokes is chairman of the Department of Geology at the University of Utah, Salt Lake City.

warmer than normal. This seems at first sight to be an astonishing conclusion. The reasoning may be fallacious and should not be accepted without much investigation and consideration, but we may pursue the idea a little farther. Where were these warmer areas? Were temperatures, integrated over the whole earth's surface, actually so different from those of the present as has been assumed? We might propose the hypothesis that climatic conditions were far from steady in any one area, but were subject to large shifts, and that intervals of ameliorated conditions in some regions coincided with increased severity in others. The Pleistocene, then, may have been a period of sharper contrasts of climate and of shifting climates rather than a period of greater cold."

Some Climatic Prerequisites

Glaciers cannot form without precipitation, and it is obvious that most of the precipitation must be in the form of snow. However, the mere fact that snow falls in large quantities on a given area does not automatically guarantee that glaciers will result. There must be an excess of snowfall over melting in order that ice accumulation can take place.

Two factors are involved, and their relative effects are more important than the magnitude of either. During periods of glacial advance any of the following conditions can result in ice formation: (i) total snowfall remains constant but potential melting falls off; (ii) snowfall increases and potential melting remains the same; (iii) snowfall increases and potential melting increases at a slower rate; (iv) snowfall diminishes and potential melting decreases at a faster relative rate. Similar but opposite conditions can be visualized for periods when ice is diminishing.

The annual increments resulting from any of the conditions just noted can multiply with time until ice masses of continental proportions are created. The important thing is not the magnitude of the annual contributions but their ultimate number. The initiation of a glacier may be a relatively insignificant event. A slight temperature decrease or a small increase in average precipitation, if effective at the right time, will be sufficient to start an ice age. Every glacier had its inception from a thin layer of snow that remained on the ground where none had remained the summer before. The temperature need suffer no drastic decline and the precipitation no large increase at the time this happens. The important thing is that the trend be unidirectional and remain in effect for a very long time, so that short-term gains are not wiped out by reversals of one sort or another. Some mechanism that either increases the pre-

cipitation or lowers the temperature very gradually over a period of thousands of years would seem to be required.

Glaciers are in general retreat today mainly because of a decrease in precipitation accompanied by an increase in temperature (5). It is thought by many that the fluctuations observed within historic time are due mainly to variations in precipitation and that glaciers could flourish in spite of present warmth if snowfall were sufficient.

If decrease in precipitation is suspected of being the most important factor in eliminating glaciers at the present time, it is possible that an increase in precipitation may have been the most important factor in producing the glacial oscillations of the past. Heavy precipitation and cold are incompatible in long-term effects, and low temperature is therefore antagonistic to glacier growth. If lowered temperature and increased precipitation are considered to have been strictly simultaneous in origin, one could not have caused the other, and outside agents or agencies must be invoked to cause both. If, however, the increased precipitation and the fall of temperature are not strictly simultaneous, then it is possible that one may have caused the other.

In my opinion, it is reasonable to imagine and possible to prove that the fall of temperature could be caused by an increase of precipitation. This requires only the assumption that there be an annual increment of snow from which ice can form. The eventual creation of large masses of ice and the dispersal of cooling effects by circulating air and water systems is an inevitable chain of events. The creation of glaciers by simultaneous increase of precipitation and fall of temperature appears to be possible only on a small local scale and seems to require intervention of extraterrestrial influences of some sort.

We are too far removed in time from the onset of major glaciation to be able to judge the actual sequence of climatic events and have made the natural assumption that the fall of temperature and increase of precipitation were simultaneous and both due to the same outside cause.

Ocean-Control Theory

Preceding sections of this article have shown the desirability, if not the necessity, of finding the cause of glacial oscillations in long-term changes in precipitation traceable to thermal variations in the ocean. Brief mention has also been made of some of the fundamental data on which such an explanation must rest and the essence of an ocean-control theory may now be stated in more precise terms. The theory depends on the unique

specific heat characteristics of water and its latent heats of freezing and evaporation. As one consequence of these properties, the temperatures of bodies of water tend to be very stable and to change relatively slowly. The ocean has such tremendous volume that many thousands of years are required to cool or heat it by ordinary means. The lag in response of the ocean to thermal change allows it to remain warm while cooling influences are in operation and to remain cool while warming influences are operating. It is the thesis of this article that the thermal lag of the ocean is responsible for the oscillation of cold and warm climates during ice ages.

The ocean receives heat from the sun and, unless it is disturbed by outside influences, must remain in thermal equilibrium with solar heating. Such an equilibrium has apparently been in effect during long periods of crustal quiet that have existed in the geologic past. The equilibrium or near-equilibrium has been disturbed during several shorter intervals by changes attendant to widespread mountain building and continental uplift. The last thermal upset during the Quarternary was marked by four superposed cycles of shorter duration. Uplift of land areas allowed the initial accumulation of glacial ice, and this in turn had a cooling effect on the atmosphere and ocean which initiated a chain of events known collectively as the Quarternary Ice Age.

Cold water from glaciers enters the rivers and cools the surface layers of the ocean. Chilled water near the polar regions tends to sink and spread into the lower latitudes, so that the bottom waters are likewise chilled. Eventually conditions are right for the formation of sea ice in the north polar regions, and icebergs from this source and from the land areas exert further cooling influences. With periodic overturns, which may be expected from time to time, the whole ocean experiences a lowering of temperature (6). The climatological effects of this cooling are complex, but the most notable is a decrease in evaporation. This gradual and slow decline is closely followed by a decrease in precipitation over most of the earth. As a consequence of this, the glaciers gradually waste away and largely disappear; inland lakes dry up and other signs of aridity are common.

While the ocean is still cold from the glacial effects, the land receives and retains more heat and the river waters run warmer than when they are derived partly from ice. Solar heat commences to assert itself, cloud cover diminishes, water area of the ocean is expanded over shallow continental shelves, icebergs and eventually sea ice disappear, albedo (reflection) effects fall off, and the ocean commences to warm. After a lapse of

time measured in thousands of years, the evaporation and precipitation again show an increase, and there is a rather sudden rebirth of the glaciers, provided that topographic conditions are still favorable to creation of snow and ice fields.

The process is repeatable and periodic as long as precipitation provides an increment of snow from which ice can form. If the topography becomes sufficiently subdued either by erosion or by isostatic adjustments, it is apparent that rain will fall where snow had fallen before, and there can be little storage of ice to bring on another cycle of cold. Ocean temperatures will now be dominated by ordinary solar heating. The terminating condition of glaciation, like its initiation, is topographic and, in the final analysis, is governed by the interaction of orogenic and epirogenic forces within the earth and degradational forces on its surface.

Glacial Cycle Analyzed

Any system showing cyclic changes, such as the stages of the Quaternary Ice Age apparently do, should exhibit at any time a predisposition of components capable of carrying it from the preceding stage into the succeeding stage. In other words, a relationship must exist that allows prediction of the succeeding effect and reconstruction of the preceding cause. If this is true, the present world climatic conditions, if properly understood, may contain the key to what has already happened and what will happen. The complete cycle need not be observed if any of it is correctly interpreted. Figure 1 shows in diagrammatic fashion the conditions in four selected stages of the glacial cycle.

An analysis of the ice age may best commence with the present, for which relatively more data are available. The present obviously lies somewhere between a minimum and a maximum of glaciation, and the glaciers appear in general to be wasting away. It may be argued that we are in a minor recessional phase and that the general trend is toward an increase of ice, but for the purposes of this discussion this makes little difference. The oceans are relatively cold, possibly about as cold as they can become. As is shown by paleotemperature studies, they are surely much colder than they were during the Cretaceous and much of the Tertiary and probably colder than they were during the period when glaciers were advancing.

Evaporation and precipitation are relatively lower than average, as is witnessed by the retreat of the glaciers, the low volume of rivers, the absence of lakes in regions of internal drainage that were filled a few thousand years ago, and the distribution of plant and animal life.

The polar ice is diminishing at a rapid rate, and it is possible to foresee the time when there will be none at all. As is the case with land ice, the present melting of the polar ice cap may represent a minor oscillation on either a waxing or waning phase of glaciation, but the weight of evidence points to the possibility of a complete disappearance.

Although the most recent trend of climate is distinctly toward warming, climatologists are not convinced that this trend will continue for long. We may have reached the minimum of glaciation during the so-called "climatic optimum" of 4500-2500 B.C., or we may see even greater warming in the centuries ahead. There is no doubt that previous inter-

glacial stages were warmer than the present, and it may therefore be assumed that we are in the waning stage of glaciation.

During the typical interglacial phase, conditions are significantly different from the stage of glacial retreat. The ocean is still cold from the effects of the recently melted ice, but it will begin to warm later in the interval. It is not possible to know just when the warming influences commence to gain the ascendancy, but it could be while there are still many glaciers in existence, probably including the Antarctic icecap.

The total period of warming, as will be shown later, would seem to require at least 50,000 years. When ice is at a minimum, the rivers run at or near their

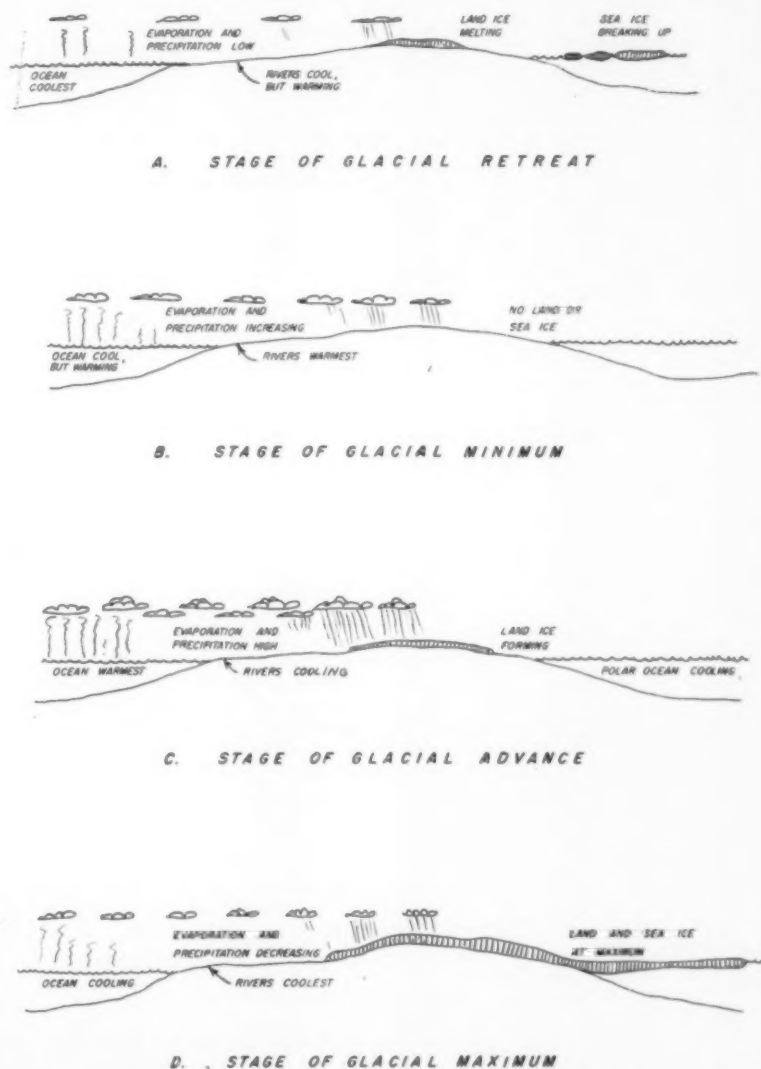


Fig. 1. Diagrammatic interpretation of climatologic elements at four selected stages of the glacial cycle.

warmest stages. Although the total volume of rivers may be rather low at first, it will increase as the cycle advances and the oceans warm. Icebergs will be few or nonexistent. The polar pack ice will have entirely disappeared, and the Arctic Basin will cease to contribute cold water to the deeper parts of the Atlantic. With the disappearance of the land and sea ice, the atmospheric zones of frontal activity that are responsible for much of the precipitation will probably weaken and shift poleward. The final removal of the ice may be a rather sudden event, for the last thin remnants may melt away or break up during a single season. All the climatic zones will shift northward, and milder climates will prevail over broad areas formerly dominated by cold.

Evaporation and consequently precipitation will be low. This will be due not only to coldness of the ocean but also to diminished windiness. Hot, dry conditions will prevail over broad continental areas. Interior lakes will be relatively low, and saline deposits will form. Loess and dune sand may accumulate over broad areas where vegetation has not established itself.

Conditions are right at this time for the gradual passage into the third stage, that of glacial advance. Preparation for the actual creation of the ice sheet may require a relatively long time. The gradual warming of the ocean may take at least 100,000 years to reach the point where the precipitation is great enough to leave a summer increment for ice formation. Once a large, permanent snow field is created, the climate may change rather rapidly, and there will be a great speeding up of the ice-forming process. The atmosphere at and over the snow and ice will be chilled and will affect the temperature of a wide belt of adjacent land. This will allow the snow cover to expand at an accelerated rate. More important will be the evaporating effects as the chilled air is carried over adjacent relatively warm oceans. The cold dry air will be capable of receiving much water vapor, which can be precipitated almost immediately. Windiness will increase, and eventually a zone of frontal storminess will be created with consequently greater precipitation on the edges of the ice sheet.

Meltwater from the snow and ice fields will commence to enter the ocean, and its most significant effects will be in the partially landlocked Polar Basin. Freezing of sea water is facilitated in many ways (7), but actual creation of a floating ice-cap must await the chilling of practically the entire body of subjacent water. The chilled surface water sinks because of greater density, but at about 37.2°F no further density differences exist, and at this crucial point sea ice will form (7, p. 112).

The cooling of the polar ocean may

take a long time, but it will be aided by the increasing amounts of cold, fresh water from the land. By the time conditions are right for polar ice formation, the land ice may have already reached the ocean over a broad front, with icebergs adding to the cooling effects. Once initiated, the polar icecap will spread and thicken with great rapidity, and the areas of sinking water will be forced outward along its periphery into the Atlantic where they are now located.

These hypothetical events are different from those postulated by Bell (8), who believes that the polar sea ice must form first and that this pre-cools the ocean in preparation for the land glaciation. Brooks has analyzed the influence of a polar icecap and has shown that the final effects appear superficially to be out of all proportion to the initial causes (2, p. 40). The ultimate lowering of the winter temperature brought about by an initial small fall of temperature of 0.6°F will amount to about 45°F. The contribution that the present theory makes is a mechanism whereby the original small temperature decline may occur; the rest of the process follows Brooks' analysis.

As long as precipitation exceeds melting, the glaciers will continue to advance; and as long as temperatures are low enough, sea ice will continue to freeze in polar regions. Four times during the Quaternary the ice sheets advanced to very nearly the same positions in central North America and then melted away. The catastrophe of almost complete smothering of the continent by ice was prevented by reversal of the process of accretion, either by a rise of temperature or by a fall of precipitation. The direct and indirect cooling of the ocean by ice would seem sufficient to lower evaporation and precipitation enough to bring about starvation of the glaciers without intervention of any outside agent.

The self-regulating process will suffice to starve the land ice, which will disappear first, perhaps irregularly and in patches, and the sea ice later when temperatures rise to the point where sea water does not freeze. During the period of glacial retreat the climate may be very unsettled. Adjustments in ocean currents, shift of wind belts and storm tracks, and overturns in the ocean may be expected. This is the climatic situation of the present time, with the stage set for the complete disappearance of the ice for a lengthy interglacial period.

Sufficiency of Causes

Most glacial theories have failed for one of two reasons: either they did not fit the facts of distribution and chronology or the postulated agents were of in-

sufficient strength to bring about the observed effects. The ocean-control theory must be subjected to rigorous examination on both of these points before it can be considered a worth-while idea. The correlation with observed facts has been discussed in a general way, and some consideration must now be given to the sufficiency of the postulated mechanisms to bring about an ice age. Many factors are involved, some of which can be evaluated in a general way, and others scarcely at all with present information.

The ocean is calculated to have a total volume of about 300 million cubic miles (9). The annual evaporation from the ocean is equal to about 80,000 cubic miles, and of this about 24,000 cubic miles falls as precipitation on the land. Of this land precipitation, about 6500 cubic miles returns to the ocean through the rivers. This is a relatively small amount, but during a century a total of 650,000 cubic miles is recirculated. Assuming for very rough calculations that the average river water is 5°C warmer than the average ocean water, a condition that might easily obtain when the ocean is coldest and warming influences are operating on land, it would require 45,000 years for the rivers to raise the temperature of the ocean by 4°C. This is roughly the time required to recirculate a volume of water equal to the total content of the oceans with present river capacity.

If the temperature difference between river and ocean water is maintained at only 1°C, then 100,000 years would be required for a 4°C change. It is well to point out that all ocean water can be classified according to the place of origin at the surface and that the properties acquired at the surface are most important in tracing the movements of the component parts of the oceans. The effects of river waters in determining salinity and temperature of areas of the ocean surface have probably not been entirely determined but are certainly important.

The cooling of river water by glaciers is only one of the effects that ice has in lowering the temperature of the earth. Ocean water is cooled by direct contact with ice or cold air and tends to sink and spread along the ocean bottoms. When the continental glaciers are discharging directly into the ocean along thousands of miles of coastline, the refrigerating effects must be tremendous. The summation of these influences would seem sufficient to cause a lowering of ocean temperatures perhaps by as much as 10°C.

When the glaciers have disappeared owing to lack of precipitation, no special mechanism is needed to restore warmth to the ocean other than normal solar heat. The rivers merely help to transmit to the ocean some of the heat that falls upon the land.

There are certainly a great number of

modifying terrestrial influences, perhaps including some totally unsuspected ones, that exercise important effects, but it appears safe to conclude that those just mentioned are operating within time periods of the right order of magnitude and along lines capable of achieving the observed effects.

Prelude to the Pleistocene

The ocean-control theory would be open to serious criticism if it made no correlation with climatic conditions before the first continental glaciation. The trend that culminated in Pleistocene ice formation may have had its inception millions of years before the actual ice age. The Cretaceous period was characterized by wide, shallow epicontinental seas, low-lying lands, and mild climates. Fossils indicate that dinosaurs lived almost to the Arctic Circle and that semi-tropical vegetation grew in Alaska. The mean temperatures of late Cretaceous oceans in the Gulf Coast of the United States and near England and Denmark were nearly uniform, as is shown by study of oxygen isotopes, and were about 15° to 16°C, considerably higher than they are at present (10).

Although ocean currents must have been quite different during the Cretaceous by comparison with the present, a difference in circulation is not needed to explain the warmer oceans and lands. During the Cretaceous there was considerable folding and faulting, but there were few really high mountains to generate ice, and the Canadian shield was relatively low. Judging by paleogeographic maps, the area of ocean exposed to the sun was perhaps 10 to 15 percent greater than it is at present, thus allowing greater heating.

During the Tertiary, a gradual cooling of the ocean and continents took place. This is shown by the study of near-shore marine organisms along the Pacific border of North America by Durham (11), by the study of land floras of the western United States by Chaney (12), and by the study of open ocean faunas of the Pacific Ocean by Emiliani (13). Similar cooling of Europe is indicated by the studies of Kerner as quoted by Brooks (2, p. 135). Without going further into details, it is evident that the time interval from the Paleocene to the Pleistocene was in general one of increasing cooling and decreasing precipitation.

The cooling was simultaneous with increasing emergence of North America and by elevation of its interior portions. The same is true of other continental masses in varying degrees. The paleogeographic maps of the late Tertiary show world-wide evidences of emergence, as compared with earlier periods.

Although the Rocky Mountain or Laramide revolution is popularly supposed to have occurred at the transition from the Cretaceous to the Tertiary, it has become increasingly evident that mountain building was continuous from place to place from the late Jurassic or early Cretaceous and that deformation continued through the early Tertiary and Quaternary. In spite of extensive folding and faulting in the early Tertiary, the western United States remained relatively low at that time. The Green River lake that occupied many thousands of square miles in the western United States during the Paleocene and Eocene is thought to have stood less than 1000 feet above sea level and to have formed in tropical or subtropical climates (14). The corresponding beds today are 5000 feet above sea level. The Oligocene and Miocene witnessed truly widespread movements, which brought about regional uplift of much of western North America. The Florissant lake beds of Oligocene age in Colorado, which have an abundant flora and fauna, are thought to have been deposited between 1000 and 3000 feet above sea level (15). These beds are at present about 9000 feet in elevation. It is thought that the uplift from the initial position to 6000 feet happened before the mid-Pliocene and the remainder since that time.

What took place in the continental shield areas during this time cannot be determined, but it may be concluded that the entire North American continent became higher and more extensive than it was at any other previous time. The continental emergence, cooling of the oceans, and decreasing precipitation on land are probably not unrelated events. Behind the climatic effects lie diastrophic causes. The creation of larger and higher continental masses could have influenced the ocean temperatures in many ways. The decrease in water area exposed to the sun's rays would have had some cooling effects; the increased radiation of heat from the elevated tracts would represent a loss of heat to the earth as a whole; and, finally, the change in temperature of the river waters could have culminated in cooling the ocean by many degrees.

The suddenness of the onset of glaciation at the beginning of the Pleistocene may be more apparent than real, for there could have been both mountain and continental glaciers of some sort immediately before this time, the effects of which have been totally erased. It may be that the numerous sharp uplifts that came about in North America and other parts of the world were sufficient to insure the formation of ice fields where none had existed before. This would entail no increase in precipitation, only a lowering of temperature through uplift. The possibility of actual increase of pre-

cipitation is suggested by the fact that the Panama land bridge connecting North and South America made an appearance, or rather reappearance, late in the Pliocene. This by itself could have had the effect of diverting increased amounts of warm water to more northerly regions or of permitting higher temperatures and evaporation in the Gulf of Mexico with consequent great precipitation in areas to the north.

The conditions at the beginning of the Pleistocene are difficult to reconstruct, and at the present no entirely satisfactory explanation for the onset of glaciation is apparent to me.

Importance of North American Glaciers

Most of the facts regarding paleotemperatures and Tertiary climatic changes that have been appealed to in this article have been derived from North America or nearby oceans. It is neither scientific nor safe to ignore the remainder of the earth, but there are good reasons to believe that the North American glaciers may have played a major, if not absolutely essential, role in the glaciation of the rest of the earth. This is because the North American glaciers constituted such a large proportion of the total volume of Pleistocene ice.

According to Anteys (16), there was 27,050,000 cubic kilometers of ice in the North American glaciers at the maximum of the fourth glacial period and a grand total of 36,850,000 cubic kilometers for the entire earth. Daly (17) gives comparable figures of 22,600,000 cubic kilometers for North America and a world total of 34,300,000 cubic kilometers. A very detailed analysis by Flint (3, p. 435) gives a total of 28,426,545 cubic kilometers for North America and a world total of 49,621,804 cubic kilometers.

It is evident that from one-half to two-thirds of the total world ice was in the North American glaciers. This means that the glacier-making process was most favored on this continent. According to the ocean-control theory, glaciation once begun is a self-regulating process, and it seems probable that the effects of the great North American ice sheet could have governed the glaciation of the smaller ice-covered areas of the North.

A thorough analysis of the thermodynamics of North American glaciation cannot be made at present, but interesting possibilities exist. At the north is the Polar Basin, and adjoining it to the south is the Canadian Shield, constituting a tremendous cold-producing area. When the glaciers are mature, some of this cold is conveyed southward by water of the Mississippi drainage, which in glacial times would be much larger than it is

now. The Gulf of Mexico lies to the south and is a great warmth- and moisture-producing area. From it and nearby oceans springs the Gulf Stream, which carries warmth into the polar areas. It seems likely that much of the precipitation for the ice fields came from the Gulf of Mexico. The thermodynamics of this system, if fully comprehended, may explain almost by themselves glaciation of the Pleistocene.

Some Supporting Arguments

Arguments for the ocean-control theory to this point have taken the form of correlated facts and inferences that directly support it, but some attention must also be paid to possible advantages it has to offer over competing theories. A few of the most widely accepted contemporary ideas should be briefly considered.

The Milankovitch theory (18) is essentially astronomical in aspect and in the recent past has enjoyed the support of many scientists (19, 20). Without going into detail, it is assumed that the combined and coincident effects of periodic changes in the eccentricity of the earth's orbit, the longitude of the equinox, and the inclination of the earth's axis to the ecliptic will determine the amount of radiation received by each point on the earth's surface. When these effects are plotted, they are believed to show maxima and minima corresponding to the changes of climate of the Pleistocene.

The Milankovitch theory has been largely discarded, in America at least, as an adequate explanation of the glacial oscillations of the Pleistocene. Arguments against it have been summarized by Flint (2, pp. 403-404, 506-507) and are obviously sufficient to cast serious doubt upon its validity.

Although Milankovitch's theory may not satisfy many scientists, it is nevertheless true that astronomical theories are more popular than "terrestrial" ones. In the recent symposium on climatic change (21), the weight of authority and the general tone is toward a solar control mechanism.

Granting that solar variations may be the cause of the ice ages, the great weakness of dependent theories is the lack of proof that sufficient changes could or do occur. Theoretical considerations supporting the possibility of solar variations of major proportions have been put forth (22), but actual observations spread over a significantly long period are not at hand. For practical purposes, a theory based on solar variation may not be susceptible to the necessary observations, and the problem will be removed from all but theoretical investigation. It seems evident that we are entitled to look with some degree of skepticism on astronomi-

cal theories as they are presently constituted.

Most recent writers are agreed that more than one factor is probably necessary to bring on an ice age, and that one of these factors is elevation of the land. This, however, fails to account for the oscillations within the general glacial period; geologists have found no evidence of significant topographic changes corresponding with the minor glacial stages other than those traceable to the action of ice itself.

The most popular present theory is that of Flint, which combines the terrestrial effects of mountain building with the astronomical effects of solar variation (2, pp. 512-550). According to Flint, the following items of basic data must be considered. (i) During glacial ages the regional snowline was lowered roughly parallel with itself. (ii) The Pleistocene climatic changes affected and are continuing to affect the whole world at the same time, and the pluvial climates coincided with them. (iii) The climatic zones in the Northern Hemisphere maintained their relative positions throughout the Pleistocene epoch. (iv) Glaciers are genetically related to mountains and other highland areas. (v) Solar energy is observed to fluctuate at present through a small range. From these facts it is deduced, among other things, that "lowered temperature, not increased precipitation, started the glacial age." This, if true, would prove fatal to the ocean-control theory, which assumes that increased precipitation preceded the formation of ice and that the refrigeration followed soon thereafter.

The postulated chain of events by which increasing precipitation (with elevated land) could cause fall of temperature is fairly simple and straightforward. The increased snowfall would produce ice almost immediately; and, if the ice masses became large enough, their cooling effect upon the atmosphere, land, and ocean would become inevitable. If, on the other hand, the fall of temperature came first, it is very difficult to see how an increase of precipitation would follow, especially on a large scale and as a cause-and-effect phenomenon.

With regard to the snowline, it should be borne in mind that its position is determined by two things, the temperature and the amount of snow. Flint (2, p. 32) gives some impressive data showing how the snowline is influenced by the total snowfall. It seems permissible to assume, on the basis of present evidence, that the increased precipitation not only could precede the fall of temperature but also is the most likely thing to occur.

Gilbert N. Lewis (23) has approached nearer to the idea of ocean control of glaciation than any previous investigator known to me. Lewis visualized the im-

mediate cause of a great glaciation as an increase in evaporation but made no specific suggestion as to how this could come about, except by accident such as the upthrust of a portion of the ocean bottom. He suggested that the termination of glaciation could be brought about by a general lowering of temperature by loss of solar energy through excessive reflection from snow and ice fields. Lewis made no provision for self-perpetuation or cyclic mechanisms and thus failed to account for the four stages of the Pleistocene Ice Age.

Conclusions

Serious and perhaps fatal objections to an ocean-control theory of glaciation will probably have occurred already to some who have read the foregoing summary. Criticisms and suggestions will be welcome. The theory is susceptible to mathematical analysis on many points. Quantitative data on the heat budget of the ocean could be brought to bear on the subject, and the relative temperatures and volumes of rivers and the bodies of water into which they flow could also be examined. Myriads of climatologic, meteorologic, oceanographic, biologic, geologic, and topographic details should be considered in any attempt to solve the problems of glaciation, always with the aim of integrating an acceptable overall hypothesis.

The ocean-control theory requires a rather clear-cut course of events during the progress of a glacial-interglacial cycle. These postulated events can be checked against what is known or what may be learned about the chronological sequence of geologic events during the Pleistocene. The theory requires a warm ocean coincident with the early and mature stages of glaciation. This rather novel concept is in opposition to most previous theories, which postulate world-wide cold affecting both the land and the ocean simultaneously. Means of checking the coincidence of warm oceans and cold lands appear to be offered by study of fossils and coral reefs, and usable results may already be at hand in this field. It is apparent that some reliable means of assuring contemporaneity of basic data from land and sea deposits must be introduced before results can be conclusive.

The theory requires a coincidence of fluvial periods and glacial advances, with the filling of interior basins running slightly ahead of the growth of the ice sheets. The times of most severe continental cold should be during the waning stages of the glaciers, and the cold periods should overlap into the early part of the succeeding interglacial period. The loess-forming intervals may be mainly late in the glacial stages.

Crucial tests based on the foregoing considerations and also many others can be devised to test the possible validity of an ocean-control theory. I intend to pursue some of these tests as they concern the geologic aspects of the problem.

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C. P. Berkey, Pioneer in Engineering Geology

Geologic knowledge has been applied intuitively to the building of man-made structures for centuries. But only in recent years has the geologist become a member of the teams that plan and construct great dams, bridges, and aqueducts. Charles Peter Berkey was foremost among those who demonstrated the value of geologic advice in the construction of public works. Born on a farm in Indiana on 25 March 1867, he moved with his family to Texas and then to Minnesota, where he completed his doctorate at the University of Minnesota in 1897. His first work was on rocks and fossils in Cambrian sandstones at Taylors Falls on the St. Croix River. But in ensuing years his interests turned to petrology, the study of igneous and metamorphic rocks, which he had seen through his college days only as erratic boulders in the glacial drift of southern Minnesota. In 1903, he came to Columbia University.

The City of New York undertook the surveys for the Catskill Aqueduct in 1906. The geologic advisers to the city at the time were James F. Kemp and William O. Crosby, professors at Columbia and Massachusetts Institute of Technology. In that summer they left for Mexico City to attend the excursions and meetings of the International Geologic Congress and were otherwise engaged. Hence, independent responsibility fell on Berkey.

The practical problems were investigated, and the answers were given with such clarity and success that he was to be engaged in such undertakings for the succeeding half-century, until his death on 22 August 1955.

Charles Peter Berkey was associated with scores of engineering projects. The Catskill aqueducts of New York and the water supply systems of Boston and Los Angeles gained his attention. He advised on the dams of the Tennessee Valley Authority and of the Bureau of Reclamation; the Hoover Dam owes its present location largely to his judgment of the merits of the foundations and tunnel conditions at this and alternate sites. The George Washington Bridge across the Hudson River bears his name on its tablets. His reports had influence on the construction of public works in half of the states of the United States and some in foreign lands. The responsibilities were a reflection not only of his good judgment and experience but also of his facility in presenting in speech and writing the nature and causes of the problems that might be encountered in such manner that his advice could be understood; hence, design and construction were altered to meet the conditions that he described. Berkey made geology a useful tool. The American Society of Civil Engineers elected him an honorary member in 1941, and the Geological Society hon-

ored him at its dedication of the *Berkey Volume on the Application of Geology in Engineering Practice*.

Berkey was Newberry professor of geology in Columbia University in his later years. His courses in petrology were particularly valued; his lectures and discussions emphasized the reasons for things—he was skilled in showing the favorable and unfavorable aspects of possible solutions and the considerations that led to his conclusions. In his most distinctive course, the students examined the rocks and sections he had used in reports on hundreds of practical problems that he had investigated. He was executive officer of the department of geology for many years, and his counsel was valued on administrative committees in the university. One of his earliest experiences in exploration was an expedition in the Uinta Mountains, Utah, about the turn of the century. But his greatest satisfaction came from the expeditions of the American Museum of Natural History in Mongolia in 1922 and 1925; much publicity was gained from the latter trip through the finding of dinosaur eggs.

One of his greatest services was as secretary of the Geological Society of America through 20 years, from the time when it was a small organization with limited resources to that when it became more richly endowed through the benefactions of R. A. F. Penrose in the early 1930's. His judgment and enthusiasm were undoubtedly largely influential in this development of the society, which elected him president in 1941. He was a member of the National Academy of Sciences and of the geological societies of London and France. The Kemp medal was awarded him in 1951 for distinguished service in geology. His greatest monuments are the enduring structures that grew under his skilled advice. His influence will affect the lives of generations who will not know his name.

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News of Science

Muller's Paper Barred

During recent weeks considerable publicity has been given to the report that H. J. Muller of Indiana University was not permitted to present a paper at the International Conference on the Peaceful Uses of Atomic Energy that was held in Geneva 8-20 Aug. From newspaper accounts based on interviews with Muller, statements by officials of the United Nations, and statements from Atomic Energy Commission spokesmen, the facts appear to be as follows:

Muller, Nobel laureate and professor of zoology at the University of Indiana, was invited—as were many others—to submit a paper for the Geneva conference. This he did.

In a letter dated 18 July Muller was told by the AEC that the U.N. had “not requested” his paper and that because of size limitations of the United States delegation he could not be included. Four days later another letter from the AEC to Muller contained an apology for the late notice and blamed the U.N. for the delay.

Before the two letters to him were sent, Muller had left for Europe. He attended the Geneva conference, as a spectator, but did not present his paper and was barred from taking part in the discussion because he was not an official delegate.

More than a month after the conference began, Warren Unna of the *Washington Post and Times-Herald* reported additional facts. He obtained from U.N. headquarters in New York a flat denial that the U.N. had barred or “not requested” Muller's paper and the further statement that the U.N. had been instructed by the AEC, in a letter dated 30 June, that Muller was not a delegate and that the AEC did not want his paper on the program.

Confronted with the U.N. statements, the AEC then admitted that it was responsible for the omission of Muller's paper. It appears that the paper was initially reviewed and accepted by the AEC technical staff that processed the conference papers. But after the paper was forwarded to the U.N. for oral presentation, it was decided by the AEC

that mention of the Hiroshima bombing made the paper “definitely inadmissible” at a conference devoted to the peacetime use of nuclear energy.

Accordingly, the U.N. was requested by the AEC to omit the paper and was informed that Muller was not a delegate. At the time this explanation was given, the AEC also declared that no suppression of Muller's views was involved since his paper would appear in the proceedings volumes to be published in a few months by the U.N.

In a statement made to the press on 3 Oct., Lewis L. Strauss, chairman of the AEC, expressed his regret that the “snafu” had occurred and declared that he assumed full responsibility for it. He added that the correct procedure would have been to ask Muller to omit mention of Hiroshima. The mistake occurred, he indicated, because the AEC technical staff had had to handle so many papers in such a short time.

The Soo and the Suez

The Soo Canal, which links Lake Superior and Lake Huron, celebrated its centennial year during this past summer. The Soo long has surpassed all other canals in volume of traffic; frequently it has been cited as having an annual volume greater than the combined tonnages of the Suez, Panama, and Kiel canals. However, in 1954 the Soo—properly named the St. Marys Falls Canal—lost its position to the Suez. Ironically, this occurred in the canal's 100th year of operation and just a year after the establishment of an all-time tonnage record of 128.5 million net tons.

The Suez outranked the Soo by a wide margin; comparative records of 106.8 and 85.4 million net tons give a difference of 21.4 million tons. Last year marked the first time that the commodities moving through the Suez exceeded 100 million net tons, whereas in 6 of the last 10 years the Soo volume has passed this figure.

This change in positions was primarily the result of the change in traffic in two commodities—iron ore and oil. In 1954 iron ore represented 73 percent of the

Soo tonnage, and petroleum, both crude and refined, accounted for 65 percent of the volume through the Suez.

In 1954 the petroleum products that passed through the Suez reached a new peak of more than 69.95 million net tons. This was also greater, for the first time, than the iron ore movement through the Soo, which totaled only 62.6 million net tons. Although the oil figure represents a gain of more than 7 million net tons over the 1953 total, the volume of iron ore declined by 36 million tons from the previous year.

Further expansion of the Middle East oil fields, which have had a spectacular development since World War II, largely accounts for the Suez Canal's increase. However, the decrease in shipments in the Soo was caused by economic factors.

The Soo ore traffic in 1953 reached an all-time high of 98.6 million tons. This record flow was the result not only of the high tempo of industrial activity in 1953, but also of the 1952 steel strike, which had delayed ore shipments. In the first half of 1954, however, steel production was only 71.5 percent of rated capacity, and this declined to 64 percent in the third quarter of the year. Accompanying this decrease in demand was a record import of 17.7 million net tons of ore. These are the conditions that caused the cargo traffic in the Soo to drop to the lowest volume since the 1930's except for 1946.

While iron ore has always dominated the Soo traffic, the rise of petroleum to its dominant place in the Suez trade has been a post-war phenomenon. In 1946 this commodity group amounted to about 9.3 million net tons, or 38 percent of the total traffic between the Red and Mediterranean seas. Only 8 years later the volume had increased 7½ times to account for two-thirds of the total cargo tonnage.

Pipe lines have had an important effect on the commerce of both the Suez and Soo. Lines from Saudi Arabia and Iraq to Lebanon on the Mediterranean Sea divert large quantities of crude oil from the Suez route, whereas the Soo is affected by the pipe line from Duluth-Superior to Sarnia, Ontario, which opened in January 1954. From 1951, when the pipe line from the Alberta oil fields reached the head of the Great Lakes, until the Sarnia extension opened in 1954, the crude moved eastward in tankers. The 1953 volume reached 3.4 million tons, but fell to one-fifth of this amount last year.

In comparing the two canals, it should be noted that because of ice the Soo locks cannot operate for more than 9 months a year, whereas the lockless Suez never closes. Based on the figures for 8

months, the Soo cargo volume for 1955 is estimated at from 107 to 110 million net tons, indicating that this year the tonnage competition between the Soo and the Suez will probably be the closest in history.

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Livonia, Mich.

Structure of Vitamin B₁₂

The articles that virtually complete the deciphering of the chemical structure of vitamin B₁₂, first announced in tentative form in 1954, appear in the 20 Aug. issue of *Nature*. The research is reported by the same two groups of biochemists that reported the earlier work. One team of six persons consisted of Dorothy C. Hodgkin, Jenny Pickworth, and J. H. Robertson of Oxford University; K. N. Trueblood and R. J. Prosen of the University of California at Los Angeles; and J. G. White of Princeton University. The other team, also comprised of 6 workers, was made up of R. Bonnett, J. R. Cannon, A. W. Johnson, I. Sutherland, and A. R. Todd, of Cambridge University; and E. L. Smith of the Glaxo Laboratories, Middlesex. Still other groups have aided in the elucidation, in particular the research group at the Merck Laboratories.

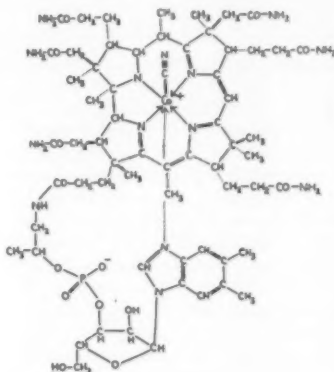
The size of these research teams, and their international composition and geographic distribution on opposite sides of the world, speak volumes with regard to the present-day organization of scientific effort. The simultaneous achievement of essentially the same conclusions by diverse methods not only points up the keenness of scientific competition in active biochemical areas, but also affords a vivid example of the way in which scientific knowledge depends on mutual confirmation.

It is by now well known that the essential structure of vitamin B₁₂ represents a new type of compound ring structure similar to, and yet significantly different from, the tetrapyrrole ring structure of the porphyrins such as chlorophyll, heme, and the cytochromes. The vitamin B₁₂ structure likewise consists of four linked rings each of which is composed of 4 carbon atoms and one nitrogen atom; but each ring has at least one tetra-substituted carbon atom and therefore lacks the typical double-bond structure of the pyrrole ring.

As in the porphyrin structure, the four rings in the vitamin B₁₂ molecule are joined by three —C— bridges, but the final, closing linkage that unites rings A and D is thought to be a direct one. The single cobalt atom of the molecule occupies the center of the tetra-ring structure, like magnesium in chlorophyll and

iron in the hemes and cytochromes. It bears a cyanide group; hence the name *cyanocobalamin* is sometimes applied to the vitamin.

Electron density maps and crystallographic data, as well as chemical analysis, now show more detailed features of the attached side chains on the ring. Three acetamide and three propionamide and six methyl groups are attached to the rings and two methyl groups to the carbon bridges. Ring D bears another propionic acid side chain, which is combined with a propanolamine residue that forms an ester linkage with the phosphate group of a nucleotide that is also coordinated with the cobalt atom. Both groups agree on the formula C₆₃H₉₀N₁₄O₁₄PCo, arranged as shown here.



To work out the details of structure of so complicated a molecule is truly a triumph of scientific ingenuity. It opens the way to greater insight into the metabolic activities of this vitamin that prevents pernicious anemia and which was isolated for the first time only in 1948.—B.G.

News Briefs

■ The Atomic Energy Commission has announced that the Los Alamos Scientific Laboratory will use the Nevada Test Site beginning about 1 Nov. 1955 for a series of experiments to determine the safety of various weapons and experimental devices in the event of accidents such as fires during handling of storage. Laboratory calculations and previous experiments have established a strong probability that such accidents will not cause nuclear detonations, but confirmation through field tests is desired.

There will be detonations of conventional explosive materials. All explosions, even if there should happen to be a nuclear detonation, will be of low explosive force. Because fissionable ma-

terials are involved, precautions will be taken; tests will be made only under carefully selected weather conditions. No off-site radioactive fallout problem is anticipated. The explosions may be heard by nearby residents. Because the detonations will take place in daylight, it is improbable that they will be visible off-site.

It is possible that even very low scale detonations such as those planned may release enough radioactive material into the air to affect the very sensitive instruments or processes of certain industries and research institutions. For this reason, the AEC will announce the conclusion of the experiments.

■ Scientists of the U.S. Department of Agriculture's Animal Disease Laboratory on Plum Island, N.Y., report success in growing the virus of foot-and-mouth disease in cultures of swine or bovine kidney cells. This accomplishment, by H. L. Bachrach, W. R. Hess, and J. J. Callis of the laboratory staff, makes possible the use of practical tissue-culture techniques in (i) diagnosis of the disease and identification of the type of virus present; (ii) determination of concentrations of the viruses and antibodies produced in animals; and (iii) large-scale production of the virus for fundamental studies and vaccine investigations.

Foot-and-mouth disease virus has been grown experimentally in other types of cultures by investigators in the Netherlands, but the methods used were not adapted to the measurement of virus and antibody concentrations. The method developed at Plum Island, which has also been independently achieved at a research laboratory in England, permits the rapid enumeration of viruses and antibodies. This work is also the first in which kidney cells from hogs and cattle have been used for routine production of virus in the quantities needed for research purposes. The method is similar to that employed for growing human polio virus for the manufacture of vaccine.

Announcement of work at the Plum Island Laboratory is the first report of research conducted inside the United States on foot-and-mouth disease, a potential major threat to the nation's swine, beef, and dairy herds. Before establishment of this laboratory, no research on foot-and-mouth disease virus was permitted in this country.

■ Robert J. Hasterlik, associate director of Argonne Cancer Research Hospital and a participant in the Geneva nuclear conference, recently reported his impressions of Soviet biology to a meeting of University of Chicago alumni. Emphasizing that contacts with the Soviet biologists who attended the conference might

not provide an accurate picture of all Soviet nuclear medicine, Hasterlik pointed out that basic differences as well as similarities in the American and Soviet approaches could be seen at Geneva.

Soviet biologists have concentrated more on the effects of radiation on the central nervous system than have scientists in this country. They have also devoted much attention to the use of radioactive cobalt-60 in cancer therapy and are installing cobalt units in hospitals across the Soviet Union. They did not report on the use of radiocobalt in rotating therapy.

Soviet scientists have been working for at least 6 years on the use of radiophosphorus in the treatment of such diseases as leukemia and polycythemia vera. In addition, radioiodine is used to study the thyroid gland and radiosodium to measure the circulation time of blood.

More directly committed to practical ends, the Soviet scientists do not appear to be as active as U.S. scientists in the uses of radioisotopes as tracers in studying basic physiological mechanisms, particularly those involving the use of radio-carbon and tritium.

Soviet science in general tends to stress one problem at a time; consequently Soviet research men do not appear to have investigated the wide number of radioisotopes that are at present under study in the United States. No papers on radiation sickness in small animals were presented, nor were there any reports of studies of protective factors in radiation sickness. Hasterlik was impressed with some of the Russian studies on the use of radioisotopes in agricultural research, such as those concerned with the behavior of tagged fertilizers.

Hasterlik reported that Soviet scientists were friendly and eager to discuss scientific problems with colleagues from other countries. The discussions appeared to be free, interesting, and fruitful from the American point of view. In general, the scientific equipment the U.S.S.R. exhibited was comparable to the kind of equipment used in the United States.

■ The first contingent of the British Commonwealth Trans-Antarctic Expedition, led by Vivian Fuchs, will leave Britain on 14 Nov. It will be followed early next year by a New Zealand group that is to be directed by Edmund Hillary.

■ The U.S. Atomic Energy Commission announced recently that it is carrying on its major research effort in controlled thermonuclear reactions at Princeton University and at AEC laboratories operated by the University of California at Los Alamos, N.M., and Livermore, Calif.

In addition, there are projects at Oak Ridge, Tenn., and New York University. The over-all program is known as Project Sherwood.

This long-range program, to which reference was made in August during the Geneva nuclear conference, has been under way since 1951; it is directed toward the possibility of controlling the release of the great amounts of energy from reactions involving the fusion of light nuclei.

In essence, the problem is that of heating an appropriate nuclear material (such as deuterium) to temperatures of several hundred million degrees and of confining it somehow at that temperature for a sufficiently long period of time to allow an appreciable portion of the nuclei to fuse together, with consequent release of energy.

Scientists in the News

AUGUST PI SUÑER, Spanish physiologist and director of the Institute of Experimental Medicine at the University of Caracas, Venezuela, has been awarded the 1955 Kalinga prize of £1000 for his work in popularizing science in Spanish-speaking countries. The prize is awarded annually by the United Nations Educational, Scientific and Cultural Organization. It is supported by a grant made by B. Patnaik of the Indian State of Orissa, who established the prize both to recognize competent interpretation of science to the general public and to strengthen links between India and other nations. Kalinga was an Indian empire that was invaded more than 2000 years ago by the Buddhist emperor Asoka. Asoka was so deeply impressed by the horrors of war that he resolved never to wage war again.

This year the jury was composed of Abdel Rahman, professor of astronomy at the University of Cairo; J. L. F. Brimble of the United Kingdom, editor of *Nature*; and Cortes Pla, chief of the Division of Science and Technology of the Organization of American States. Born in Barcelona in 1879, Pi Suñer joined the University of Caracas as a professor of physiology after a career of teaching and research in Spain. In 1922 he received the Achucarro national prize in Spain for his research in the physiology of the nervous system and in 1948 he was awarded the Prix Pourat of the Paris Academy of Sciences for his book, *The Vegetative Nervous System*.

In addition to his scientific work, Pi Suñer is the author of a series of books intended to bring science within the grasp of the layman. Among his works which have been translated into English are *The Bridge of Life* and *Classics of Biology*.

L. J. F. BRIMBLE, editor of *Nature*, returned to London on 15 Oct. after a month in the United States. He visited scientific centers and individual scientists in Boston, Chicago, Ithaca, New York, Princeton, and Washington.

C. B. LARRABEE, long an executive of *Printers' Ink*, will become director of publications for the applied journals of the American Chemical Society on 1 Nov. The new position was created by the ACS board to facilitate coordination of the increasingly complex publishing activities of the society.

Walter J. Murphy, editor of the ACS applied journals since 1943, will become editorial director of the journals. This is also a new position; its title describes Murphy's responsibilities more accurately than that of "editor."

The four journals concerned, which have a combined circulation of more than 150,000, are *Chemical and Engineering News*, weekly news magazine and the society's official publication, and the society's three monthly journals—*Industrial and Engineering Chemistry*, *Analytical Chemistry* and the *Journal of Agricultural and Food Chemistry*. Larrabee's primary responsibility as director of publications will be that of coordinating the advertising, circulation, circulation promotion, and editorial programs of the four journals.

N. HOWELL FURMAN, professor of chemistry at Princeton University, delivered the seventh annual Friend E. Clark lectures that are sponsored by the Tau Chapter of Phi Lambda Upsilon at West Virginia University. The lectures are given in honor of F. E. Clark, former professor of chemistry and department head at the university. Furman's work is concerned with analytic chemistry, polarography, coulometry, and potentiometric titrations.

EMILE F. HOLMAN has been honored with a *Festschrift* issue of the *Stanford Medical Bulletin* (August 1955). The special issue contains 25 articles contributed by colleagues and former students. Although he is continuing his private practice and research, Holman retired on 1 Sept. as head of the department of surgery at Stanford Medical School.

In addition, a commemorative issue of the *American Journal of Surgery* was recently dedicated to Holman. Presentation was made at a surprise breakfast given by more than 100 associates.

D. J. HANKINSON, former head of dairy industry at the College of Agriculture, University of Massachusetts, has been named head of the new depart-

ment of dairy and animal science, which combines the former dairy industry and animal husbandry departments.

VICTOR A. RICE, formerly chief of animal husbandry and a faculty member for 39 years, retired on 1 Sept. to become director of instruction in the School of Agriculture at North Carolina State College, Raleigh.

LEE LORCH, formerly professor of mathematics at Fisk University, has been appointed professor of mathematics at Philander Smith College, Little Rock, Ark.

IAN MCMILLAN, of St. Thomas' Hospital, London, England, gave a lecture on 19 Oct. at Vanderbilt University School of Medicine. McMillan has made some unusual films on the motion and function of heart valves from inside the heart chambers.

BERNARD S. J. WÖSTMANN, assistant director of the Netherlands Institute of Nutrition and the Laboratory of Physiological Chemistry, University of Amsterdam, has been appointed biochemist on the research staff of the Lobund Institute, University of Notre Dame.

ARTHUR M. BUSWELL resigned on 1 Sept. as chief of the Illinois State Water Survey, which he has headed since 1920. Under Buswell's leadership the survey has developed into one of the largest units of its kind in this country, with a staff of 58 scientists and technicians. Buswell has accepted a position as research professor in the department of chemistry at the University of Florida, where he will participate in an expanded program of water research.

OLIVER H. GISH has been appointed visiting professor of physics at Southern Illinois University for the academic year 1955-56. From 1922 until his retirement in 1948, Gish was on the staff of the department of terrestrial magnetism of the Carnegie Institution of Washington, first as physicist and later as chief of the section of terrestrial electricity and as assistant director of the department. Since his retirement, he has been part-time consulting physicist to the U.S. Air Force and the U.S. Navy Mine Defense Laboratory.

JOHN EISELE DAVIS, pioneer in the development of recreation for the treatment of mental patients, retired from the Veterans Administration on 30 Sept. after 35 years of Government service. One of the first books on recreational therapy published in this country was written by Davis in collaboration with William Rush Dunton, associate profes-

sor of psychiatry at Johns Hopkins Medical School. The book appeared in 1933.

Davis is the author of five books and several manuals on the subject of medical rehabilitation, corrective therapy, and recreational therapy as psychotherapeutic-activity approaches. He has also written more than 100 articles, both technical and popular. His most recent book, *Clinical Applications of Recreational Therapy* (1952), was published both in this country and in England.

He was the founder of the Association for Physical and Mental Rehabilitation. In 1946, while he was conducting special training courses for corrective therapists in cooperation with Karl Menninger at Topeka, Kans., he founded the Association for Physical and Mental Rehabilitation. The organization now has chapters in every state.

Davis attended the University of Richmond in Richmond, Va., and Washington College in Chestertown, Md., where he received his M.A. degree. In recognition of his contributions to the field of physical and mental rehabilitation, the college later awarded him the degree of doctor of science, *causa honoris*.

He has taught, and assisted in the organization of, special courses in rehabilitation at Columbia University, New York University, and Springfield College. One of his contributions was his assistance in the development of the Roland technique for reaching the so-called "mentally dead" patients with a form of relaxation therapy that has been accepted as an addition to modern psychiatric treatment.

HERBERT SPRINCE, chief research biochemist at the U.S. Veterans Hospital in Coatesville, Pa., and visiting lecturer in biochemistry at New York Medical College and Flower and Fifth Avenue Hospitals, has recently been appointed research associate in psychiatry at the University of Pennsylvania. In his new capacity, Sprince will continue with his clinical biochemical studies of indole metabolism in schizophrenia at the Coatesville hospital.

A. E. NEHRENBURG and PETER LILLYS, both of the Crucible Steel Company of America, Harrison, N.J., have received the American Society for Metals 1955 Henry Marion Howe medal. They were honored for their joint paper on "High-temperature transformations in ferritic stainless steels containing 17 to 25 percent chromium."

WILLIAM JUSTIN KROLL, a native of Luxembourg, and an American citizen since 1950, is this year's winner of the society's Albert Sauveur achievement award. He was recognized for his pioneering work on malleable titanium.

JOHN M. HAMILTON, associate professor and head of the biology department at Park College, has been appointed acting dean of the college. He assumes the post left vacant last spring by the resignation of E. McClung Fleming. He will continue teaching and will remain head of his department.

EDMUND B. TUCKER, who for the past 5 years has been a member of the faculty at the University of Minnesota, has become a research associate at the General Electric Research Laboratory, Schenectady, N.Y.

MAURICE H. GREENHILL, formerly professor of psychiatry at the University of Maryland School of Medicine, has been appointed head of the department of psychiatry at the University of Miami School of Medicine and director of the Psychiatric Institute.

The following awards were announced by the American Society for Horticultural Science at its annual meeting in September:

HENRY M. CATHEY of Cornell University, the Alex Laurie award in floriculture and ornamental horticulture.

B. LENNART JOHNSON of the University of California, Los Angeles, the Leonard H. Vaughan award in floriculture.

OTMAR SILBERSTEIN of Westfield, N.Y., the Charles G. Woodbury award in raw products research.

C. M. GERALDSON of Manatee Station, Bradenton, Fla., the Leonard H. Vaughan award in vegetable crops.

E. L. PROEBSTING, JR., of Irrigation Experimental Station, Prosser, Wash., and E. L. KENWORTHY of Michigan State University, the J. H. Gourley award in pomology.

The following scientists received honorary degrees from the Polytechnic Institute of Brooklyn on 8 Oct. during the closing convocation of the institute's centennial year.

LINUS CARL PAULING, Nobel laureate in chemistry and chairman of the division of chemistry and chemical engineering at California Institute of Technology; ALEXANDER KARTVELI, vice president and chief engineer of Republic Aviation; THOMAS E. MURRAY, member of the Atomic Energy Commission; MERVIN J. KELLY, president of Bell Telephone Laboratories; OLE SINGSTAD, designer and builder of the Holland Tunnel; FREDERICK W. ZACHARIASEN, chairman of the department of physics at the University of Chicago; EGER V. MURPHREE, president of Esso Research and Engineering Company; PHILIP SPORN, president of American Gas and Electric Company; LLOYD V. BERKNER, president of As-

sociated Universities, Inc.; ERICH HAUSMANN, dean emeritus of the Polytechnic Institute of Brooklyn; HAROLD M. MORSE, mathematician, Institute for Advanced Studies; ERNEST PAYSON GOODRICH, engineering consultant; FRANK L. BABBOTT, former president of the Long Island College of Medicine; and ERNEST VAN NORDEN, retired engineer for Consolidated Edison, Inc.

The following appointments to assistant professor have been announced. Stanford University: FRANK R. ARNOLD, mechanical engineering. University of Oklahoma: MAURICE K. TEMERLIN, psychology. University of Massachusetts: FRANK E. POTTER, dairy chemistry.

Necrology

AARON B. BAGSAR, Drexell Hill, Pa.; 58; retired metallurgical engineer; 7 Oct.

FRED W. FITZ, Chicago, Ill.; 57; associate professor of medicine at Northwestern University; 9 Oct.

ERNEST P. GOODRICH, Brooklyn, N.Y.; 71; civil engineer; former professor of engineering economics at New York University; president of the American Institute of Consulting Engineers in 1951; 9 Oct.

DAVID W. HENRY, Toledo, Ohio; 70; retired dean of the University of Toledo; 12 Oct.

JOAN HOPKINS (Mrs. David M.) Los Altos, Calif.; 27; geologist, coauthor of a forthcoming publication on slope erosion to be issued by the Geological Society of America; 3 Oct.

HENRY JORDAN, Pasadena, Calif.; 80; retired expert on azo dyes at E. I. du Pont de Nemours Company, Deep Water, N.J.; 5 Oct.

RAYMOND C. OSBORN, Columbus, Ohio; 83; professor emeritus and former chairman of the department of zoology and entomology at Ohio State University; 6 Aug.

HENRY C. SHERMAN, Hastings-on-Hudson, N.Y.; 79; professor emeritus of chemistry at Columbia University and one of the nation's leading nutritionists; 7 Oct.

KENNETH B. TURNER, New York, N.Y.; 54; cardiologist; associate professor of clinical medicine at the College of Physicians and Surgeons, Columbia University; 9 Oct.

Education

■ Four members of the staff of the College of Agriculture at Ohio State University have arrived in India, where they will spend the next 2 years assisting in the development of agricultural education, research, and extension pro-

grams. Thomas S. Sutton, assistant dean of the university's College of Agriculture, heads the project, which represents a joint undertaking with the International Cooperation Administration. Others from the university are Everett L. Dakan, department of poultry husbandry; J. P. Schmidt, department of rural sociology, Agricultural Extension Service; and Charles L. Blackman, dairy science extension.

The contract between Ohio State and ICA is the first of five with land-grant colleges in the United States. Other contracts are to be signed with the University of Illinois, University of Missouri, University of Tennessee, and Kansas State College.

The Ohio group will be joined later by a soils expert, a horticulturalist, and a highway engineer. The American visitors will work in the northwest portion of India, which includes the states of Punjab, Rajasthan, and Himachal Pradesh. A primary concern of the ICA program in India is to increase food production.

■ In celebration of its centennial year, Berea College conducted a program on 6 and 7 Oct. entitled "Atoms at work." The speakers included Hubert N. Alyea, professor of chemistry at Princeton University; Cyril Comar, principal scientist of the medical division, Oak Ridge Institute of Nuclear Studies; Merlin D. Peterson, professor of chemistry and head of the department of chemistry at Vanderbilt University; Thomas Strickler of the Berea department of chemistry; and W. G. Pollard, executive director of Oak Ridge Institute of Nuclear Studies.

■ A second special course in radioisotope techniques for foreign scientists and technicians opened at the Oak Ridge Institute of Nuclear Studies on 17 Oct. with 30 students from 20 countries participating. So many applications were received from interested foreign candidates for enrolment in the first special course, held in May for 32 students from 21 countries, that another session was set aside for those who applied but could not be accommodated earlier.

The special course is identical with the regular course that is given by the institute six times a year. Fundamentals of radioisotope use are taught during the 4-week intensive training period. The participants learn how to use and calibrate radiation detection instruments, how to purify and separate radioactive materials from inert or other radioactive materials, and how to apply them to a variety of chemical and biological research problems. More than 2000 men and women have received this training, under ORINS, since it began in 1948.

Grants, Fellowships, and Awards

■ The United Nations Educational, Scientific and Cultural Organization has an annual fund of \$17,000 for assistance to research projects on arid lands problems. Grants from the fund are made on recommendation by the Advisory Committee on Arid Zone Research. The committee's next meeting is scheduled for 7-10 Nov. in Paris. Details and application forms are available from Division of Scientific Research, Department of Natural Sciences, UNESCO, 19 Avenue Kléber, Paris, France.

■ The School of Medicine and Dentistry at the University of Rochester has announced the availability of eight U.S. Atomic Energy Commission fellowships in industrial medicine for 1956-57. The fellowships are open to men and women physicians who are citizens of the United States, who have graduated from an approved College of Medicine at least 2 years prior to beginning tenure of the fellowship, and who are licensed to practice medicine in one of the states or territories of the United States. Successful candidates will be required to have a full FBI background investigation and to receive clearance from the AEC prior to award of a fellowship.

The training program consists of two parts: an academic year, with lecture and laboratory instruction, and an in-plant training year in which the fellow will be assigned to one or more of the medical departments of the major operating plants and laboratories under the direction of the Atomic Energy Commission.

Applications for the academic year 1956-57 should be filed before 1 Jan. 1956. It is expected that the selection of fellows will be made on or before 1 Feb., but fellowships may be assigned at any time at the discretion of the Committee on AEC Fellowships in Industrial Medicine.

The stipend during a fellowship or academic year is \$3600. The sum of \$350 is added to the stipend for a wife, and \$350 more is added for each dependent child. Tuition and laboratory fees, which would be required of students of similar university status, will be paid in academic courses. Certain other expenses incident to the work of the fellow will be paid when approved by the committee. During the in-plant year the stipend will be \$6000.

The fellowship year of academic training may be taken at a university offering an approved graduate course in industrial medicine that can provide the special training facilities necessary in the study of the health problems associated with atomic energy. The in-plant year of training will be given at AEC contractor installations such as Oak Ridge,

Tenn., or Los Alamos, N.M. Address all inquiries to Dr. Henry A. Blair, Atomic Energy Project, University of Rochester School of Medicine and Dentistry, Rochester 20, N.Y.

■ The Southern Fellowships Fund, acting for the Council of Southern Universities, Inc., is offering a program of fellowship awards and grants-in-aid for advanced study or research in institutions of higher education in the following states: Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia. The primary purpose of the program is the advancement of teaching and scholarship in colleges and universities in the southern area. The type of grant and the scope of award included in the program may vary from time to time to meet changing needs and opportunities.

For 1956-57, fellowship awards will be made to qualified persons who plan to carry on advanced research leading to the Ph.D., or similar high degree, primarily in the basic biological and physical sciences, the social sciences, or the humanities. Grants for study in professional fields will ordinarily be made only when such study is to be made in preparation for a teaching position.

Application should be made directly to the fund. Completed forms must be mailed *before 15 Dec.* to the executive director, Robert M. Lester, 119 North Columbia St., Box 427, Chapel Hill, N.C.

Miscellaneous

■ *Yale Conservation Studies* (vol. 4) has been released by the Yale Conservation Club of the Yale Conservation Program, 77 Prospect St., New Haven, Conn. The plan of the Yale Conservation Program is to acquaint people from diverse professions with the broad concepts of land and water use that have grown out of the various sciences during the last few decades. The aim is to give each conservation student a thorough understanding of the basic principles involved in the use and care of natural resources and competence in applying those principles to actual problems.

The *Yale Conservation Studies* is an annual publication of the students' Conservation Club. The essays are written by conservation students and friends of the program in other departments of the university.

■ Science and engineering vacancies in UNESCO technical assistance and technical aid programs have been announced by UNESCO's New York office. Salaries

generally range from \$6000 to \$8750 a year, free of national income tax. Should the expert have a family, he receives a dependent's allowance of \$200 a year for his wife and a children's allowance of \$200 a year for each child. Lodging is furnished by the host government or a lodging allowance is paid in lieu thereof.

Travel expenses are paid to duty station and back. They are also paid for his wife and dependent children, if his contract is for 1 year or longer. Unless otherwise specified, initial contracts are for 1 year, with the possibility of renewal in many cases. For additional information, write to Mr. Arthur Gagliotti, Executive Officer, UNESCO, United Nations, New York 17.

■ A group of nine hydrologists from four Asian countries and various international agencies has just completed a 130-page glossary listing and defining some 1000 terms used by engineers, scientists, and others to describe the occurrence and movement of water. The glossary gives precise meanings for such terms as *rain-fall*, *ground water*, and *river and stream flow*; it is described as a kind of dictionary that should provide a basis for common professional language among Asian hydrologists in planning new river-basin development projects in flood-control and irrigation programs.

The original draft of the glossary was prepared by the Bureau of Flood Control of the U.N. Economic Commission for Asia and the Far East (ECAFE), following a recommendation of the first Regional Water Resources Development Conference organized by ECAFE in Tokyo in May 1954. In its final version, the glossary is the product of work by a group of hydrologic experts who met in Bangkok from 12-26 Sept. under the joint auspices of the secretariats of ECAFE and the World Meteorological Organization. The specialists came from India, Japan, the Philippines, and Thailand; they were joined by experts from ECAFE and WMO and also by representatives of the World Power Conference and the International Commission on Irrigation and Drainage.

The compilation of an Asian glossary of hydrologic terms, which is in English, is part of an effort toward the drawing up of a similar glossary that would be acceptable throughout the world. The glossary will be published early next year as a part of ECAFE's flood-control series.

■ An appeal for the return of lost radiosondes was made recently by the U.S. Air Force Air Weather Service and the U.S. Weather Bureau.

Each radiosonde carries a label informing the finder of its value. If taken to the nearest post office, the set will be shipped to a repair center without cost

to the finder. Cost of a new set is about \$35, while the cost of rehabilitating a set is between \$6 and \$7, including postage.

The Air Weather Service headquarters in Washington, D.C., reports that approximately 8 percent of all the radiosondes it released in the first 3 months of this year were returned to be repaired and reused. It is hoped that the figure can be raised to 25 percent.

■ The American Philosophical Society has from time to time made grants to individuals for apparatus to be used in connection with research, with the understanding that the apparatus will be returned to the society after it is no longer in use for that particular research. As a consequence, the society now has a number of items of apparatus. Any institution or individual interested in acquiring an item should request a list of the available instruments from the executive officer of the society, L. P. Eisenhart, 104 S. Fifth St., Philadelphia 6, Pa.

■ The Institute of Animal Resources of the National Research Council is interested in learning of the existence of private bibliographies on subjects concerning animals. Titles of the bibliographies will be published as a part of the *Handbook of Laboratory Animals*.

The information desired is as follows: title of bibliography; name and address of collector; number of references; whether annotated; whether bibliography is inclusive; whether bibliography has been published; if not published, whether it can be seen. Please address information to Mr. Berton F. Hill, Institute of Animal Resources, National Research Council, 2101 Constitution Ave., Washington 25, D.C.

■ New York's first major exhibition on the peaceful uses of atomic energy is being held through 3 Nov. at the Carnegie Endowment International Center, United Nations Plaza, New York. Sponsored jointly by the Atomic Industrial Forum, the Carnegie Endowment for International Peace, and the Fund for Peaceful Atomic Development, the exhibit entitled "Man, the atom and the future" will stress the peacetime uses of atomic energy.

The exhibition will consist of two major sections, a technical section and an industrial section. The technical section will be highlighted by the official United States technical exhibition that was shown at Geneva by the Atomic Energy Commission during the last summer's nuclear conference. More than 80 firms have helped develop and equip the exhibit. The show's industrial section will display models and atomic materials of more than 30 companies actively engaged in the atomic energy field.

Reports and Letters

Bias in the Allocation of Treatments by Random Numbers

When an investigator wishes to test the significance of differences between the effects of two experimental treatments, he needs an unbiased method of allocating the treatments to the various units of his material (animals, human beings, test tubes). The purpose of this paper is to make better known a simple method of allocation and to compare it with a popular but potentially hazardous method (1).

Random method of allocation. The recommended method of allocation may best be illustrated by an example. Suppose it is desired to assign 20 subjects to treatments A and B, respectively. The 20 subjects are represented by serially numbered index cards. The random numbers table is entered at an arbitrary point, and two-digit random numbers are read, moving along horizontally. (In the case of Fisher and Yates' tables (2), the random numbers may also be used in the vertical direction.) Index card No. 1 receives the first two-digit number, index card No. 2 the second, and so on. The cards are then sorted in ascending order of random number (00 regarded as 100). If the subjects are to be equally divided between treatments A and B, those represented by the first 10 cards may receive treatment A and the remaining subjects treatment B (or vice versa). Duplicates, triplicates, and so forth are ignored unless they fall at the boundary between treatments. For instance, the tenth and the eleventh cards may bear the same random number. Then the choice regarding which of the two should go into the treatment-A sample can be made by drawing two numbers, one for each of the cards, from another block of random digits after stipulating that the higher number shall correspond to, say, treatment A. (In case of triplicates, three additional random numbers must be drawn.) If one wishes to allocate subjects to more than two treatments, one simply breaks the array of subject cards, which are sorted in ascending order of the affixed random numbers, into as many equal parts as there are treatments, and assigns a different treatment to each part. The method can also be applied when the parts are unequal but of assigned size.

Biased method of allocation. If two treatments are to be compared, but there is no stipulation regarding the equality (or other ratio) of numbers of subjects, it is perfectly legitimate, although possibly inefficient, to decide by coin tossing which treatment shall be applied to each subject; or, for greater convenience, one may use odd and even random numbers. Frequently, however, this method has been applied in equal-number experiments, by allocating randomly until half the subjects have been assigned to one of the treatments and then assigning the remainder automatically to the other treatment. Under those conditions, the tossing of a coin with equal head-tail probability, or the equivalent "odd-even" random numbers method, yields non-random allocations such that significance tests tend to be misleading, because what is believed to be, say, the 5-percent level of significance (that is, 5 percent of wrong verdicts when chance alone is operating) may be something quite different in reality.

Illustration of bias. Table 1 shows the percentage of wrong verdicts "significant" in 200 samples allocated by the odd-even method and in 300 samples allocated by the random method for sample size 20, with equal numbers of subjects on imaginary treatments A and B. The theoretical values for the odd-even method are found by probability techniques (3) that will be submitted

elsewhere for publication. To test results in the case of Fisher's "exact" contingency test (4), which with small samples is preferred to the chi square test, we have considered four types of heterogeneity in the experimental material. Irrespective of treatment, certain specified subjects (for example, 1 to 5) are destined to die while the rest (6 to 20) live. Apparent effects of treatment are measured by death or survival. The expected percentages of wrong verdicts for the contingency tests in the case of the random method were taken from Mainland (5). Agreement between experimental sampling and expected values for the random allocation is quite satisfactory for experiments of this size. To show results in the use of the *t* test, we have chosen experimental material that contains a simple linear trend such that each serially numbered subject differs from the preceding one by plus one unit of measurement.

The proportions of wrong verdicts for the contingency tests indicate that the odd-even method produces the worst results when the end part of the allocation contains a number of individuals with characteristics different from those allocated earlier, as when subjects 16 to 20 die. (This is because the odd-even method assigns longer runs at the end of a sequence than does the truly random method.) While this bunching of like individuals may appear extreme, its occurrence in practice is, nevertheless, entirely possible when the characteristics of the subjects change with time. If a few odd individuals are scattered haphazardly through the experimental material, the odd-even method will yield results that are hardly distinguishable from those obtained by the random method. On the other hand, it is seen that, when two relevant characteristics alternate in the sample, the odd-even method actually produces fewer wrong verdicts "significant" when chance alone is operating;

Table 1. Percentage of wrong verdicts in testing the difference between two "treatments" applied to ten subjects each, using random and biased methods of allocation by means of random numbers.

Condition of material	Biased method ("odd and even")		Random method	
	Theoretical	Exptl. (200 samples)	Expected	Exptl. (300 samples)
<i>"Exact" contingency test</i>				
Subjects 1 to 5 die	6.25	8.00	3.26	3.67
Subjects 1 to 10 die	10.94	13.00	2.30	2.00
Subjects 16 to 20 die	30.18	29.50	3.26	1.33
Subjects 1, 3, 5, . . . , 17, 19 die	1.01		2.30	
<i>Student's <i>t</i> test</i>				
Subjects 1 to 20 have linear trend in measurements	Not computed	25.50	Appr. 5.00	5.00

however, this is accompanied by an increase in the wrong verdicts "not significant" when there is a real difference between the effects of the treatments.

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1. I am greatly indebted to Donald Mainland for suggesting the method, for his constructive criticisms and suggestions, and for his interest and encouragement. I am indebted to both Mainland and Ruth M. Smith for carrying out the sampling experiments. For random allocation methods also see A. L. Edwards, *Experimental Design in Psychological Research* (Rinehart, New York, 1951), p. 23.
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11 July 1955

Excretion Patterns of Rats Following Total-Body Exposure to X-radiation

Nitrogen and sulfur metabolism change radically following total-body exposure to ionizing radiation, with excretion increasing within 1 day and remaining high in nonsurvivors but decreasing after a period in survivors (1-5). Detection of early metabolic changes could be of vital importance in a national emergency (6).

Twelve young (180 ± 5 g) male Sherman-strain rats, after a 3-hour fast, were irradiated in lucite chambers with 550 r (LD_{42}^{80}) delivered from a deep-therapy G.E. Maximar X-ray machine (7). Animals were maintained at 25°C and were fed Purina Lab Chow and water *ad libitum*. Complete individual 24-hour fasting (water supplied) urine samples were collected before and at intervals after irradiation. Thirteen compounds per sample were determined in triplicate using paper chromatographic and colorimetric methods (Table 1). The individual means were averaged by groups surviving 30 (5), 14 (2), and 11 (3) days, groups I, II, and III, respectively. Each animal served as its own control owing to the wide individual variation, and all percentage changes were related back to the preirradiation-sample values of each group.

Urine volumes and the excretion of most compounds became elevated in all groups. Glycine was subnormal throughout the postirradiation period, while taurine, valine, and aspartic acid were, for the most part, subnormal after the first day. Urea and alanine were elevated during the entire period. One-day postirradi-

ation phosphate, taurine, and alanine increased, while histidine and aspartic acid decreased progressively from groups I to II to III, and the uric acids of the nonsurvivors were significantly greater than those of group I. On day 5, alanine and glycine trends remained unchanged, while group III diverged further from groups I and II by maintaining an elevated phosphate, uric acid, and creatinine and a depressed histidine. Glutamic acid and aspartic acid dropped in all.

During the acute phase, when animals were dying (days 10 to 14), urine volumes, phosphate, urea, and alanine were elevated, while taurine and glycine were depressed. The divergence between groups I and II and group III was marked on day 9 with respect to every-

thing except glycine, valine, and aspartic acid. Animals dying on day 14 (group II) were markedly different from those in group I in everything except phosphate, glycine, alanine, and taurine, which were now at about the same levels as the premortal values of group III. Creatinine and aspartic acid were depressed in group II, while group I had returned to normal levels. Uric acid increased in both. Histidine dropped in group II to the premortal levels of group III, while it steadily increased in group I. Values of groups II and III just prior to death were nearly the same for some compounds but were significantly different for others. It is apparent that some of these changes are common precursors of impending death,

Table 1. Relative percentage changes in excretion patterns of rats exposed to a total-body dose of 550 r x-radiation

Compound determined	Survival group	Post irradiation days							
		- 3	1	5	9	13	17	23	29
Phosphate	I	100	163†	83	144	201†	150	121	136
	II	100	209	64	141	208			
	III	100*§	212*†	160	214*†‡				
Creatinine	I	100	112	84	93	115	128†	107	130
	II	100	101	88	119	72*			
	III	100*§	197*†	134*	135*‡				
Urea	I	100	127	101	111	140†	149†	113	142†
	II	100	224*	139	143	161			
	III	100*	114*	144	119*‡				
Uric acid	I	100	242†	85	72	115	130	124	126
	II	100	289*†	88	63	102			
	III	100*§	288*†	164	237†				
Taurine	I	100	118†	92	60†	67†	67†	76†	96
	II	100	147*†	97	81†	69			
	III	100	162*†	80*†	66*†				
Glycine	I	100	84	67†	57†	69†	66†	71†	70†
	II	100	71†	68†	58†	71†			
	III	100	81	68	63†‡				
Valine	I	100	119†	95†	86	86†	86	86	90
	II	100	124*†	118	106	100			
	III	100	122†	94	83				
Alanine	I	100	127	155†	127	109	118	109	145†
	II	100	138	200	107	100			
	III	100	150	200	100*‡				
Aspartic acid	I	100	106	62†	62†	94	75	81	94
	II	100	94	56	62	56			
	III	100	58*†	45*†	64				
Glutamic acid	I	100	77†	74†	97†	116†	139	149	129
	II	100	86	79	90	103			
	III	100§	55*†	60*†	84*				
Histidine	I	100	97	105	122	132†	136†	226†	239†
	II	100	71	84	96	79			
	III	100	58	65	70				
Urine volumes	I	100	201†	156	123	132	123	114	103
	II	100	378†	219†	186†	186†			
	III	100*§	221†	352	182*†‡				
Body weight	I	96	95	85†	79†	81†	82†	87†	92
	II	96	95	86†	78†	78†			
	III	96	95	84†	78†				

Significantly different from the following at $p < 0.05$ (9):

* From group I on the same day. † From day 0 within the group.

‡ Final day of group III from final day of group II.

§ From group II on the same day, calculated for day 0 only.

|| Due to the 24-hour fast.

while others are more dependent upon the physiological state existing during the phase of the radiation-sickness syndrome in which death occurs. By day 29 only taurine and urine volumes had returned to preirradiation levels in group I. The relationship of some of these changes to known sequelae of the syndrome are evident: for example, taurine to depressed —SH (8) and disrupted cysteine metabolism, histidine to hemopoiesis, phosphate and uric acid to nucleic acid metabolism, and urea and creatinine to tissue damage and starvation. The other amino acid patterns are difficult to understand at present.

The biological action spectrum of ionizing radiation is very broad—the amount required to inactivate enzyme molecules is greater by several orders of magnitude than that required to kill mammals, and this in turn is much greater than that required to inactivate lymphocytes. However, the difference between the amount of radiation that is needed to give mammals either a 100- or 0-percent survival in a given period is very small. At a given dosage in this range the distinction between survival and death is very fine. A group of animals (inbred or not) irradiated with an identical dose probably vary widely at the time in relative sensitivities and recovery potentialities, owing to both genetic heterogeneity and phenotypic variability. Some individuals survive and some die. The metabolic differences among them are detectable within 24 hours after exposure. Indeed, in this case, short-term survivors had the lowest individual preirradiation values of the 12 rats in urine volumes, phosphate, creatinine, urea, and uric acid. Further study may make possible the construction of an index of survival; for example, on day 1, animals surviving only 10 days had the highest phosphate, taurine, creatinine, and alanine, and the lowest urea, aspartic acid, glutamic acid, and histidine. Taurine is especially interesting because of the small individual variability.

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6. Supported in part by a contract with the Atomic Energy Commission.
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13 June 1955

Reward Schedules and Behavior Maintained by Intracranial Self-Stimulation

Olds and Milner have demonstrated a rewarding effect produced by electric stimulation of some areas of the brain (1). Rats that could electrically stimulate themselves in the septal region and certain other areas each time they pressed a lever (continuous reinforcement) were able to maintain high lever-pressing rates without any other reward. The present study was undertaken to develop, through the use of reward (reinforcement) scheduling techniques, stable, long-term lever-pressing rates sensitive to the effects of relevant variables.

In contrast to the continuous reinforcement procedure in which every lever press produces the reward, the reinforcement may be programmed in such a way that only occasional responses are rewarded. This may be accomplished by means of a variable-interval schedule, in which the lever is primed to deliver the reward on a random time basis, or by means of a fixed-ratio schedule, in which a fixed number of responses is required to produce the reward. Such schedules have been demonstrated to generate characteristic types of behavior when conventional rewards—for example, food or water—are used (2).

A pulse-pair generator recently described by Lilly and his coworkers (3) served as the electric stimulus source. Stable lever-pressing rates have been maintained by rats and cats on reinforcement schedules over periods as long as 6 months without any change in the stimulus parameters. The stimulus, delivered through chronically implanted electrodes (4), had a frequency of 100 cy/sec and a pulse-pair duration of 0.1 msec, with amperage varying from animal to animal. The duration of each train of pulse-pairs was 0.5 sec, regardless of the duration of the lever-press. In the rats, the electrode tips were located in the septal area, while in the cats the caudate nucleus was found to be an effective site of stimulation (5).

Figure 1 presents 15-minute cumulative response curves obtained from one cat under two reinforcement schedules. The curves shown are typical of those obtained during the intervening days. On the variable interval schedule the lever

was connected to the stimulator at irregular intervals, with a mean of 16 sec, so that only some lever-presses produced the intracranial stimulation. On the fixed-ratio schedule, seven responses were required to produce each electric stimulus. The animals were originally trained on a continuous reinforcement schedule, in which every lever-press resulted in an electric stimulus. Marked differences in the rate of responding were obtained with the two schedules. The fixed-ratio was also characterized by typical pauses following reinforcements, although these are generally obscured in the reduced figure.

The curves of Fig. 1 are similar to those obtained with food or water reinforcement. However, the low ratios and short mean intervals at which responding could be maintained suggest comparison with small amounts of reinforcement (6). On the assumption that stimulus intensity may be analogous to "amount" of reinforcement, amperage was varied during an hour-long session for one cat that was producing an irregular curve on a fixed-ratio schedule of 8:1. At the start of the session the stimulus was presented at a lower amperage than was usual for this cat. Figure 2, depicting the complete record for one 60-minute session, suggests that an increase in electric stimulus intensity may act in a manner similar to an increase in the amount of reinforcement.

In addition to producing stable behavior sensitive to other variables, such as electric stimulus parameters, intermittent reward schedules also have the advantage of minimizing the influence of gross motor effects of the stimulus on the response rate. Such schedules have proved useful in studying the effects of other motivating conditions, for example,

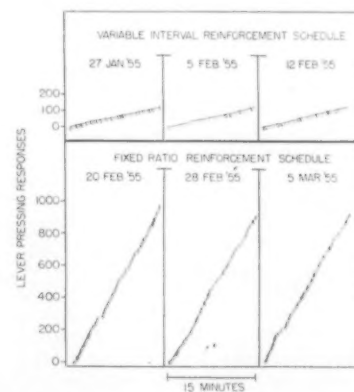


Fig. 1. Fifteen-minute cumulative lever-pressing curves for fixed ratio (7:1) and variable interval (mean of 16 sec) intracranial electric stimulation reinforcement (cat E-5). Oblique "pips" indicate reinforcements.

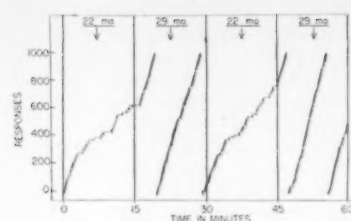


Fig. 2. A 60-minute session under fixed-ratio (8:1) during which the electric stimulus current was varied in alternate 15-minute periods (cat E-5, 7 Mar. 1955).

food and water deprivation, and conditioned "anxiety" states, on behavior controlled by brain stimulation. Reports of these investigations are now in preparation.

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21 June 1955

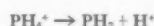
Proton Affinity of Phosphine in the Phosphonium Halides

It has been pointed out by Grimm (1) that it is possible to calculate the proton affinity of ammonia, P_{NH_3} , if the crystal energies of the ammonium halides and the electron affinities of the halogens are known. Using this method, Sherman (2) has calculated the proton affinity of ammonia in the ammonium halides and found it to be 221.0, 209.0, 208.6, and 202.7 kcal in NH_4F , NH_4Cl , NH_4Br , and NH_4I , respectively. An average value of 206.8 kcal was adopted. Similar calculations were made for the proton affinity of water, the calculated value being 182 kcal.

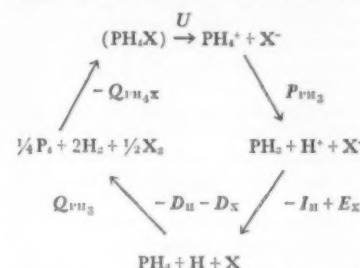
Experimental evidence indicates that phosphine is a weaker base than am-

monia. The absence of a series of phosphonium salts comparable in stability to the ammonium salts is evidence for the decreased basicity. At room temperature PH_4I is a solid (sublimation point $62^\circ C$), while the bromide and chloride are dissociated gases. Since the proton affinity of a molecule is a measure of basicity, it was of interest to calculate this value for phosphine.

The proton affinity of phosphine, P_{PH_3} , is defined as the energy change for the reaction



This energy change can be calculated indirectly by use of the familiar Born-Haber cycle. This cycle is represented as



The proton affinity at $0^\circ K$ is given by the relation

$$P_{PH_3} = U + Q_{PH_4X} - Q_{PH_3} + D_H + I_H + D_X - E_X - 5/2RT,$$

where U is the lattice energy of the PH_4X (X representing chlorine, bromine, or iodine) Q_{PH_4X} is the heat of formation of PH_4X , Q_{PH_3} is the heat of formation of phosphine, D_H is the heat of dissociation of hydrogen, I_H is the ionization potential of hydrogen, D_X is the heat of dissociation of the halogen molecule, E_X is the electron affinity of the halogen, and RT is the gas constant, $1.987 \text{ cal deg}^{-1} \text{ mole}^{-1}$, times the temperature, $298.1^\circ K$.

Table 1 gives the thermal data required to calculate the proton affinity of phosphine in PH_4I , PH_4Br , and PH_4Cl . Because of the unreliability of many of the data, the calculated proton affinities are accurate only to about ± 5 percent in PH_4I and about ± 10 percent in the other two halides. The error is of this magnitude because the crystal lattice of PH_4I is the only one known with accuracy (3). Similar structures have been assumed for the other two halides. Thus, the PH_4I value for the proton affinity would be the most reliable.

Recent electron affinity values for the halogens E_X (4) are lower by about 5 to 7 percent than the values used by Sherman (2). This would give a higher proton affinity for ammonia by about 2 to 5 percent. Thus, the new values would be in the range from 226 to 210 kcal. However, even with this revision, the

Table 1. Proton affinity of phosphine at $0^\circ K$

Quantity	PH_4I	PH_4Br	PH_4Cl
U^*	131.5†	130.3	132.2
$-Q_{PH_4X}$ (5)	15.8	29.5	42.5‡
Q_{PH_3}	2.21	2.21	2.21
$-D_H$	52.1	52.1	52.1
$-I_H$	311.9	311.9	311.9
$-D_X$	25.5	26.7	28.9
E_X (4)	74.6 (6)	81.5	86.5
$5/2RT$	1.5	1.5	1.5
$-P_{PH_3}$	200 ± 10	209 ± 21	217 ± 22

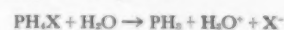
* Assume a CsCl lattice, densities of PH_4Br and PH_4Cl estimated at 1.94 and 1.27 g/cm³, respectively.

† All values in kilocalories.

‡ Estimated from the Q_{PH_4Cl} in the gas phase.

proton affinity of phosphine is of the same order of magnitude as that of ammonia.

The low value for the proton affinity of water would indicate that the H_3O^+ is less stable than the PH_4^+ . The reverse seems to be true because the phosphonium halides, unlike the ammonium halides, are readily hydrolyzed by water according to the equation



Apparently other factors must enter in, because this result is not what would be predicted according to the calculated proton affinities of water and phosphine.

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15 March 1955

Ecology and the Population Problem

In commenting on the problem of providing space and food for the growing human population, A. M. Woodbury implies [*Science* 122, 200 (1955)] that this problem is sufficiently critical in the United States to reduce such questions as those concerning the preservation of our national parks and monuments and recreation areas to the status of "minor matters." Woodbury is my former teacher and companion in fieldwork, and he is the man most directly responsible for my initial decision to become an ecologist; hence, there is no one to whom I would

listen more intently and respectfully on the solution of an ecological problem. This time, however, I must take exception, for I feel that in much of the loud discussion of the "population problem," a clamor to which Woodbury has now added his voice, at least one-half of the fundamental ecological considerations are commonly ignored.

The reasoning behind proposals such as the one under discussion is, briefly, as follows. "The human population is growing. If we extrapolate this curve of growth into the future we find that it will not be long before present facilities will be inadequate to feed or otherwise provide for the population. Therefore, we must provide new facilities." The proposals for new facilities vary from writer to writer and include the restoration of damaged agricultural land, new methods of exploiting submarginal land, increasing the harvest from the oceans, fish farming, algae farming, and various schemes for utilizing atomic energy.

Woodbury leans toward engineering feats that will make it possible to utilize lands now nonproductive because of aridity. These are all technical proposals for increasing the "carrying capacity" of the earth for human populations. Of more fundamental ecological interest are these neglected questions: Is it legitimate to extrapolate the curve of population growth? How large a population is it desirable to have? Will an increase in carrying capacity solve the population problem? These questions, I believe, deserve serious consideration in the light of ecological knowledge.

The growth of any population, plant or animal, can be resolved at least qualitatively into two phenomena, the capacity of the organisms to increase in numbers and the capacity of the environment to support organisms of that type. All species are potentially capable of increase beyond the capacity of the environment. The earth, or for that matter, the universe, is not large enough to contain all the houseflies or mushrooms that would exist after a few generations if the potential rate of increase could be sustained. The same generalization applies to elephants, whales, men, and sequoia trees; they would simply take a little longer to fill the universe.

Now, nearly all our experience with a great variety of species indicates that a population tends to grow rapidly when it is well below the capacity of the environment. The growth rate decreases as environmental resources become limited, and a population that has saturated its environment cannot, by the definition of capacity, increase at all. If the capacity of the environment is increased, the result to be anticipated is a new cycle of population growth. For this reason one may question whether anything perma-

nent is to be gained by increasing the carrying capacity of the environment, unless there are other very good reasons for regarding a larger population as desirable. In his recent textbook of ecology, Woodbury relates a report that in Shanghai, prior to World War II, 25,000 infants were annually discarded in the garbage. A population twice as large and living in the same manner would presumably discard 50,000 infants annually. Enlarging the effective environment would not of itself provide any permanent solution to what I visualize as the "population problem."

Extending this somewhat imaginative picture to the United States, we can visualize a temporary relief of population pressures while the now-arid Great Basin and Southwest are becoming as densely populated as, say, Manhattan Island and while our lakes are being transformed into algae "factories." At the end of the cycle of population growth we would not have our recreation areas but we would still have the population problem.

The life-history pattern of man is such that a human population with plenty of room for expansion is potentially capable of exponential growth at an "interest rate" of about 3 percent continuously compounded. The population of the United States was growing at about this rate in 1790. Our present-day reproductive performance is not consistent with this rate of growth; for the U.S. and for the earth the present rate of human population growth is apparently slightly more than 1 percent and less than 2 percent per year. However, it is a completely obvious but commonly overlooked fact that any positive rate of population growth must ultimately cease. The capacity of the environment will determine how many people will be present when the population ceases to grow, but even if we could expand to the point of "standing room only" there would still have to come a time when population growth would stop.

When the population ceases to grow we must have, on the average, one individual leaving the population for each one entering it. It is irrelevant for the present discussion whether the final population will fluctuate or will reach an equilibrium size or steady state. In either case the average death rate must be equal to the average birth rate; this is the only real solution on earth to the population problem. (I admit to being already too old and staid to regard interplanetary emigration as offering a promising solution to the problem.)

It is commonly regarded as desirable to have a low death rate, and the only permanent way of achieving this is to have a low birth rate. How low can this go and the population still be sustained? If, as seems imminent, we can attain a

mean length of life of 75 years, the minimum birth rate for replacement will be 1/75 birth per person per year, or 13.3 per 1000 population per year. But this is higher than any crude annual death rate experienced in the United States since 1920. Even if it should become possible to extend the average length of life to 100 years this would require, in order to maintain a stationary population, a birth rate and a death rate of 10 per 1000 population per year, which is above the U.S. rate that has prevailed since 1948. These are simple arithmetic facts, and neither socialized medicine nor the Corps of Engineers will alter them.

Here, I believe, we see an aspect of the population problem that should be aired. Inevitably there must come a time when birth rates and death rates will be equalized through the cessation of population growth. It would, however, take a brave administrator, when he was faced with a climbing death rate, to oppose any measure whatsoever proposed to combat the trend. Hence, until the population problem is considered in its entirety, we can anticipate pressures for keeping the population growing no matter what sacrifices may be required.

I have no intention of making specific proposals for managing natural resources or population growth in the United States, and I do not pretend to know what population size would be optimum. Even if I did know this, I could not say whether population stability could best be obtained by birth control, by restrictions on marriage, or by the "natural" controls such as famine, epidemics, and fertility changes, which will inevitably take over unless man finds a rational solution. The problem, however, is essentially ecological, and it can be approached objectively. I hope that it will be before too much of our natural heritage is sacrificed in the vain hope that dam-building projects and the like will solve the population problem.

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8 August 1955

A. M. Woodbury's discussion of science, population, and arid lands [*Science* 122, 200 (1955)] seems to use some premises whose validity he does not clearly establish. From them he appears to argue that a scientific approach to the question of whether "to develop the Upper Colorado River for use of part of its waters," in certain interior states, would necessarily result in an attitude favorable to the proposal. He evidently regards conservationists who oppose the plan as emotional and unscientific; employing "di-
versionary tactics," they worry about

"whether we are setting a precedent of invading a national monument, and various other minor matters." Scientists, it seems, are immune to the influence of emotion in making value judgments.

The author's citation of Paul B. Sears' editorial [*Science* 121, 5A (29 Apr. 1955)] does not appear to support his own viewpoint clearly. Pertinent references I find in the editorial are to the need, "in matters of public policy where verifiable physical knowledge is involved" (italics mine), of "impersonal, disinterested, and competent boards of scientists" and this reference to one such group: "The Engineers Joint Council has investigated and reported (unfavorably) upon the Upper Colorado project" (parentheses Sears').

Woodbury seems to imply that it is wicked to doom arid lands "to remain arid with sparse populations," but he does not show why disinterested scientists would surely find it less wicked to favor "such a proposal as turning water from Yellowstone Lake through the divide into Snake River. . . ." Nor does he show why, once all pertinent scientific data were supplied, the problem of whether to convert an arid area into an area teeming with people would be a question to be decided by scientists but not by nonscientists.

It is to be hoped that scientists will not too hastily assume that they have become the only ones competent to make a value judgment.

ALEXANDER LINCOLN, JR.
New Hampshire Representative for
the Nature Conservancy, Meredith
8 August 1955

Of the two accompanying papers commenting on my article [*Science* 122, 200 (1955)], the one by Lamont C. Cole is a very fair statement to which I do not object; the other by Alexander Lincoln, Jr., distorts some of my meanings and imputes to me assumptions that I do not accept. Nowhere did I assume that anything discussed was "wicked," or that scientists are the only ones capable of making "value judgments." His article is a good example of the semantics used by "emotional" conservationists to which I called attention in the article.

I wrote the original article because I saw so much partisanship displayed on both sides of the controversy, and I hoped to find solid groundwork between the two extremes upon which reasonable people could agree. In essence, my proposal was to have the Congress authorize development of the Great Basin in accordance with the Colorado River Compact and refer disputed problems to a body of scientists trained in the art of fact finding (borrowed from the Sears' editorial). Such problems as (i) whether reserva-

tions for reclamation purposes were made before Dinosaur National Monument was established, (ii) whether the building of Echo Park Dam would be an invasion of the national park system, (iii) whether Echo Park or some other site should be used, and (iv) other disputed points should be referred to this group for decision.

Can it be that the type of conservationist represented by Lincoln is afraid to submit these controversial questions to scientific dissection? He signs himself as the representative of the Nature Conservancy, of which I am a charter member and have devoted much effort to conservation support. I am much interested in the national forest, national park, wildlife refuge, reclamation of arid lands, flood control, and other conservation movements and will continue to devote what effort I can to maintain such movements.

Cole marshals a lot of ecological data, most of which we share in common (we worked together in the Colorado Basin) to assist in analyzing the problems of the desert. It is on the implications and inferences from such data that we appear to differ. I go a step further than he does. I thoroughly agree with his implied conclusion that available habitat has an important influence on population control of plants, animals, and man, but there is one important difference between man and the other biota that he has not emphasized.

This difference is the use of intelligence by which man is making more habitat available to himself by dispossessioning competitive plants and animals and transforming the habitat to provide more of his needs. The desert is one of the best places for making such a transformation. By diverting streams, man dispossesses fish and other stream animals of available habitat and, by putting water on sagebrush land, he not only dispossesses the sagebrush but also sage hens, sage thrashers, Brewer's sparrow, and many other sagebrush animals.

In place thereof, he substitutes homes (trees, flowers, lawns), towns or cities, industries, mines and related operations, military posts for which much space is needed, irrigated agriculture (least important at the present time), and many other more or less important items. Thus man can increase his available habitat and allow population to expand and fill it before he becomes subjected to the drastic population restrictions to which Cole has referred.

There is, however, an additional aspect that will allow further expansion. This is efficiency in utilization of natural resources. It is a race between research and population increase. If the latter overtakes the former, the population will necessarily become static, and the drastic

controls will be automatically applied. This efficiency in greater production of human needs in available habitat is being manifested on all sides: more application of physical energy (water power, carbon fuels, atomic energy, solar energy); better processes of mining; more products of industry (automation); more efficient agricultural production (fertilizers, improved strains of crop plants and livestock, hybrids such as corn and sheep); better homes (individual homes with yards, lawns, and so forth); better health (sanitation, nutrition, antibiotics, medical advice); and many others.

Developments such as these not only will allow population expansion for a long time in the future but will tend to raise the standard of living. This will mean more leisure time to be devoted to recreation, more means of travel, and more demand for recreational sites and facilities. The Colorado Basin is rich in such recreation sites. There are thousands of miles of canyons and side canyons now unutilized. Perhaps there should be a program of development in the basin for recreation, comparable to that for water. With all available water applied to the basin, only a very small percentage of the land could be improved. The basin could never be converted into an "area teeming with people" by comparison with densely populated areas (contrary to Lincoln's imputation to me).

There have already been too many misunderstandings and distortions in this controversy. Let us not add any more. I use the following assumptions. (i) From the tone of the International Arid Lands Conference in New Mexico, I interpreted it to be an accepted objective of the participants to find ways and means of making arid and desert lands more productive of human needs. (ii) The Colorado Compact was a compromise of interests that provided an approximately fair division of the water of the Colorado River. (iii) This compact should be implemented by development of the Upper Colorado River to finish the program initiated in the Lower Basin. (iv) Plans for this development were under way before Dinosaur National Monument was enlarged to include Echo Park. (v) The controversy about building the dam at Echo Park should be settled on a basis of open-minded study rather than political controversy.

Here are a few points that I think should be clearly stated. (i) The development of the Upper Colorado Basin should not be confused with the problem of Echo Park Dam. (ii) The Echo Park reservoir would not flood the dinosaur bones quarry. (iii) The assumption should not be made on the basis of available evidence that the cost of development of the Upper Colorado Basin would be excessive. (iv) Desert homes properly

supplied with water are preferred by many people to homes in more humid areas. (v) It has not yet been clearly established whether the building of Echo Park Dam would be an "invasion" of the national park system, or whether the extension of Dinosaur National Monument to cover Echo Park was an "invasion" of the reclamation program. (vi) The Colorado Basin is so rich in undeveloped scenic resources that the Echo Park region must be regarded as a relatively small part of the total recreational capacity.

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12 September 1955

6-Aminonicotinamide—a Potent Nicotinamide Antagonist

During the course of investigations on the inhibition of sulfonamide acetylation, it was observed that 6-aminonicotinamide was extremely toxic to rabbits (1). The delayed effect of the compound, the first sign of which was loss of control of the hind legs (2), suggested that 6-aminonicotinamide might be an antimetabolite of nicotinamide. This was confirmed by experiments in rats. In fact, it appears to be the most potent known antagonist of nicotinamide.

The median lethal dose (LD_{50}) of 6-aminonicotinamide (3, 4) for mice, shown in Table 1, is 35 mg/kg of body weight, as compared with 305 mg/kg for 3-acetylpyridine (5). Table 1 also shows that the simultaneous administration of 50 mg/kg of nicotinamide brings about an eightfold increase in the LD_{50} of 6-aminonicotinamide. Nicotinic acid, also, gives significant protection in contrast to its ineffectiveness against 3-acetylpyridine, when administered simultaneously with the latter (2, 5). Tryptophan appears to give some protection. The administration of 50 mg/kg of 6-aminonicotinamide resulted in 100-percent mortality within a week. When tryptophan was given simultaneously (50 mg/kg orally) with 6-aminonicotinamide, there were no deaths the first week, and 30 percent of the animals were alive at the end of 30 days.

On the assumption that 6-aminonicotinamide may give rise to an inactive

Table 1. Effect of nicotinamide and nicotinic acid on the median lethal dose (LD_{50}) of 6-aminonicotinamide in mice. Metabolite and 6-aminonicotinamide were administered simultaneously intraperitoneally. Mice: CF-70 strain, 18 to 22 g.

Metabolite	Dose (mg/kg)	6-Aminonicotinamide (LD_{50} mg/kg)	No. of mice	95-percent fiducial limits
None		35	30	33- 37
Nicotinamide	25	121	30	113-129
Nicotinamide	50	308	40	281-331
Nicotinic acid	25	75	70	64- 89

Table 2. Oxygen consumption of liver from 6-aminonicotinamide-treated mice

Substrate	Oxygen uptake (μ lit)*			
	15-min incubation		30-min incubation	
	Control	6-aminonicotinamide treated	Control	6-aminonicotinamide treated
None	55	16.5	100	30
Lactate 0.015M	69	35	129	66
Lactate 0.015M + DPN 0.002M	105	116	206	183

* Average values of duplicate vessels. Each vessel contained homogenate equivalent to 100 mg of tissue (wet weight). Homogenates were prepared in 0.25M sucrose under closely identical conditions and were incubated at 37°C in modified Krebs-Ringer phosphate.

DPN analog, it was of interest to compare the rate of oxygen uptake of tissues from treated animals with that of normal controls. The results of one experiment are shown in Table 2. In the absence of added substrate the oxygen uptake of mouse liver homogenate prepared from the treated animals was only 30 percent of the normal. Apparently the treated mice were depleted of both oxidizable substrate and DPN, since the addition of these substances *in vitro* greatly increased the rate of oxidation, while the addition of both together restored it almost to normal. The treated mice had received an intraperitoneal injection of 100 mg/kg of 6-aminonicotinamide and 25 mg/kg of nicotinic acid 72 hours prior to the experiment. No appreciable effect on oxygen uptake was observed when 50 mg/kg of 6-aminonicotinamide was used, or upon the addition of 6-aminonicotinamide *in vitro* to liver homogenate prepared from normal mice.

In view of the recent findings of Kaplan *et al.* (5) the toxicity of 6-aminonicotinamide may be due to the formation of an inactive DPN analog, with consequent depletion in certain tissues of DPN. It is of some interest that one of the pathological changes observed after

an animal had received a toxic dose was involution of the spleen, a fact that may be related to the high rate of analog formation in this organ (5). Frequently, animals survived until 20 to 30 days after the administration of 6-aminonicotinamide. This may indicate, as is suggested by Zatman *et al.* (6), irreversibility of analog formation, with consequent inability of the tissues to rid themselves of the antimetabolite. These matters are at present under investigation in this laboratory. 6-Aminonicotinamide and its congeners are also being tested for their effect on neoplastic growths.

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References and Notes

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2. D. W. Wooley, *J. Biol. Chem.* **157**, 455 (1945).
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5. N. O. Kaplan *et al.*, *Science* **120**, 437 (1954).
6. L. J. Zatman *et al.*, *J. Biol. Chem.* **209**, 453 (1954).

30 June 1955

The great desideratum for any science is its reduction to the smallest number of dominating principles.—J. CLERK MAXWELL, Matter and Motion.

Book Reviews

Crust of the Earth. A symposium. Arie Poldervaart, Ed. (Geological Society of America Special Paper 62) Geological Soc. of America, New York, 1955. viii + 762 pp. Illus. \$6.50.

In October 1954 the department of geology of Columbia University had a symposium on "The crust of the earth" to mark Columbia's bicentenary. The Geological Society of America published the symposium this July just in time for the Geophysical Year. There are 44 papers dealing with the crust of the earth, and these papers are put into four parts: "Nature of the earth's crust," with 12 papers; "Recent deformation and sedimentation," with 9 papers; "Structural synthesis and petrogeneses," with 13 papers; and "Historical development of the earth's crust," with 10 papers. The authors were selected by a committee from geologists "still actively engaged in research on the particular subject allotted to them . . . who would be readily available." The selection has brought papers from widely scattered students, both in the United States and abroad, and from university, government, and industrial geologists.

Just what limits the crust of the earth is left to the authors. However, V. Meinesz suggests the Mohorovicic discontinuity be abbreviated to *M discontinuity*, and this abbreviation is used throughout. Walter Bucher would limit the term *crust* to a chemical-petrologic meaning and suggests the term *stereosphere* for that which lies above the *asthenosphere*.

In part I Ewing and Press lead off with the geophysical contrasts between continents and ocean basins and their paper is followed by other papers that consider the geology, seismism, physics, and chemistry. Part I closes with Ahrens' "Oldest rocks exposed." Part II considers deformation and sedimentation and necessarily deals with various geographic areas. The first four papers consider very large areas such as the Pacific Ocean; others discuss smaller areas such as the Tonga Trench. In part III a good many readers will be interested in H. H. Read's "Granite series in mobile belts"; as Read says, there are granites and granites. Part IV, dealing with the his-

torical development, starts with R. C. Moore's consideration of invertebrates and the geologic time scale. Two other papers cover plants and vertebrates, and one deals with isotopic dating. Two discuss aspects of atmosphere and hydrosphere, and the last four consider the development from a nonlife angle.

This is a stimulating book. The various papers do not attempt to give the final answer to the material discussed; their purpose is to arouse interest and to show the extent of our current knowledge on the crust of the earth.

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Adrenal Cortex. Transactions of the fifth and final conference. Elaine P. Ralli, Ed. Josiah Macy, Jr., Foundation, New York, 1954. 187 pp. Illus. \$3.75.

This volume records the transactions of the fifth and final Josiah Macy conference on the adrenal cortex. The subjects reported are "The salt and water factor of the adrenal cortex," by H. L. Mason; "The metabolism of adrenal steroids," by R. I. Dorfman, and "ACTH—a single substance or a mixture of hormones," by F. G. Young.

Mason outlines some aspects of his studies that contributed to the isolation of the adrenal steroid that has since been identified by Reichstein and his associates as 11 β ,21-dihydroxy-3,20-diketo-4-pregnene-18-al, and has been termed *aldosterone*. The ability of aldosterone to promote sodium retention is approximately 100 times that of desoxycorticosterone.

Dorfman points out that the steroid hormones possessing biological activity contain in ring A, an α , β -unsaturated ketone. A characteristic feature in the metabolism of these steroids is the reduction of the ketone group to a secondary alcohol and the saturation of the 4,5 double bond. He presents evidence indicating that the stereoisomeric form that results from the reduction of the double bond is influenced by the substituent groups at carbon atoms 11 and 17.

Young opens his remarks with a brief summary of the data published by con-

temporary workers that suggest that ACTH may be two or more substances. Experimental results obtained in his own laboratory indicate that one adrenocorticotrophic substance is effective in reducing the ascorbic acid content of the adrenal glands, while another is predominantly active in increasing the weights of these organs. The presentation and discussion advance our knowledge of this interesting problem, but the central question remains unanswered. The physiological response to exogenous ACTH is modified by the rate at which it is absorbed from the injection site. C. H. Li points out that ACTH administered in saline may have little effect on adrenal gland weights but that the same material administered in beeswax-peanut oil produces a marked increase in the weights of adrenals. In view of these findings the possibility remains that the procedures to which ACTH is subjected do not fractionate it but change its physical characteristics and thus alter its rate of absorption and the physiological response.

This volume contains very little material that is of direct interest to the clinician. The speculations and suggestions arising during the discussions are provocative and should prove stimulating to those who are interested in the fundamental aspects of the biochemistry and physiology of the adrenal gland.

JAMES SALTER

CHARLES H. BEST

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Industrial Inorganic Analysis. Roland S. Young. Wiley, New York; Chapman and Hall, London, 1953. vi + 368 pp. 36s.

The author, who is employed by International Nickel Company of Canada, Ltd., New York, has collected a series of his notes on industrial analytic procedures related to 43 elements, many of them the so-called "less familiar" elements, such as beryllium, fluorine, molybdenum, niobium, tantalum, platinum metals, selenium, tellurium, thorium, titanium, tungsten, uranium, vanadium, and zirconium. In addition to these and some of the common elements, he has also included methods of analyzing for oxygen, nitrogen, and water. A section on gas analysis is placed in the last chapter on "Miscellaneous analyses and data." The book is concluded with a good list of reference books. There are adequate author and subject indexes.

The methods reported have been taken mostly from readily available journals and standard treatises. Some use has been made of company brochures like those of the Burrell Technical Supply

Company, G. Frederick Smith Chemical Company, Solvay Process Company, Aluminum Company of America, and the Dow Chemical Company, to mention only a few. Of the methods developed on the Manhattan Project, the author refers only to those that were collected by C. J. Rodden and published as part of the National Nuclear Energy Series. No direct references are given to the less readily available USAEC documents.

The procedures recommended are those that have been tried and found to work in an industrial laboratory. They are given in considerable detail, with suggestions regarding manipulative techniques, methods of standardizing reagents, and methods of computing the results. Where details are omitted, reference is made to a reliable published method. For example, the vacuum fusion method of determining oxygen in metals like titanium or zirconium is only outlined, but a good method published in 1951 in *Analytical Chemistry* is cited.

A useful list of absorbers for gases is given in Table 5, on page 324. Methods of standardizing acids and bases are included.

Although this is not an encyclopedic treatise, it is nevertheless a very valuable collection of analytic methods that work. Inorganic and analytic chemists will find it to be a very handy guide.

LAURENCE S. FOSTER

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Précis d'Ecologie Animale. F. S. Bodenheimer. Payot, Paris, 1955. 315 pp. Illus. F. 1200.

The author's point of view, as expressed in his *Problems of Animal Ecology*, (which was published in 1938), is not greatly different in this revised French version (translated directly from an English manuscript). While rejecting community concepts other than those of statistical aggregation and regarding some of the newer ideas of community metabolism as akin to Pythagorean mysticism, Bodenheimer nevertheless concedes that some of these concepts may be fruitful ways of thinking about ecological conditions. His objection is that there is a tendency to consider them to be based on scientific induction rather than as loose analogies. His primary concern is with the influence of physical and biotic factors on individuals and populations (with a minimum of mathematics), and on the whole, little seems to have been added since the original edition. Unfortunately there is no index, although there is a fairly complete table of contents.

JOEL W. HEDGPETH

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Solvents Manual. With solubility chart.

Compiled by C. Marsden, Ed. Cleaver-Hume, London; Elsevier, Houston-New York, 1954. xii + 429 pp. \$12.95.

This book contains a very useful compilation of physical and chemical data on organic solvents and related substances, a field that has expanded rapidly during the last 10 years. A great deal of information that has been released by numerous industrial organizations is widely scattered in the literature and in company pamphlets.

The author should be commended for having achieved the tremendous task of collecting all these data in a concise and easily accessible form so that the user knows where and how to find them with ease and convenience.

H. F. MARK

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Chemistry of Carbon Compounds. vol. III, pt. A, *Aromatic Compounds*. E. H. Rodd, Ed. Elsevier, New York-London, 1954. xxiv + 685 pp. Illus. \$17.50.

Volume III, part A, *Aromatic Compounds*, of the *Modern Comprehensive Treatise on Chemistry of Carbon Compounds* treats specifically the benzene derivatives. [Volume I A, *Science* 116, 181 (1952); volume I B, *Science* 117, 422 (1952); volume II, *Science* 120, 256 (1954)] Volume III B, to follow later, will be concerned with the more complex derivatives, including multinuclear aromatic compounds.

A comprehensive treatise on so broad a field as organic chemistry demands an approach that is both analytic and authoritative. Furthermore, to be of real value to the professional organic chemist, it must be up to date and complete in its reference to the pertinent literature. *Aromatic Compounds* answers these requirements in almost every detail for the benzene series. The imposing list of contributors, which includes experts in their own specific fields, adds greatly to the contribution of this outstanding work. The contributors to the present volume are C. K. Ingold, W. J. Hickinbottom, Neil Campbell, J. Chatt, R. E. Fairbairn, D. H. Hey, E. Hoggarth, Z. E. Jolles, and Gareth H. Williams.

Chapter I begins with a historical treatment of the theory of aromatic character, followed by current theories on orientation and substitution, and a section on the formation and fission of the benzene nucleus. The later chapters follow the familiar order of presentation of Richter, with chapters on the expected derivatives of benzene through the carboxylic acids. Chapter VII, "Aromatic

metal and metalloid compounds," will be of special interest to many organic chemists. Each chapter presents the methods of formation and the chemical reactions of the given class of compounds, together with references to the original literature. The most significant members of a series are presented separately with the physical constants of the original compounds and those of the most important derivatives. Pertinent literature references are included also.

This treatise on benzene derivatives will be welcomed especially by seasoned organic chemists, since it presents fundamental concepts, together with fundamental properties of the most significant members and derivatives of the benzenoid hydrocarbons. The authors have been very successful in the difficult task of maintaining a good balance among the many topics, theories, and compounds that demand treatment in a work of this kind. The inclusion of references to the original literature throughout the treatise adds greatly to its value to the research chemist. The carefully compiled index also enhances the value of the treatise. Finally, the publishers have presented an attractive volume, free from errors, well printed, and with consistently good formulas.

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Miscellaneous Publications

(Inquiries concerning these publications should be addressed, not to Science, but to the publisher or agency sponsoring the publication.)

Forest Research Institute and Colleges, Dehra Dun. Govt. of India, Ministry of Food and Agriculture, 1954. 28 pp.

Indigenous Cellulosic Raw Materials for the Production of Pulp, Paper and Board. pt. XXV, *Wrapping Papers from Acacia decurrens Willd (Wattle Wood)*. Indian Forest Bull. No. 195. R. V. Bhat and H. K. Kaushik. Manager of Publications, Delhi, 1955. 10 pp. 1s. 3d.

Old Akkadian Inscriptions in Chicago Natural History Museum. Texts of legal and business interest. Fieldiana: Anthropology, vol. 44, No. 2. Ignace J. Gelb. 177 pp. \$5. *Revision of the Hawaiian Members of the Genus Tetraplasandra A. Gray.* Fieldiana: Botany, vol. 29, No. 2. Earl Edward Sherff. 93 pp. \$1.50. Chicago Natural History Museum, Chicago, 1955.

Statistics of State School Systems: Organization, Staff, Pupils, and Finances, 1951-52. Chapter 2, Biennial Survey of Education in the United States, 1950-52. Samuel Schloss and Carol Joy Hobson. U.S. Office of Education, Washington, 1955 (Order from Supt. of Documents, GPO, Washington 25). 105 pp. \$0.35.

Efficiency and Selectivity of Commercial Fishing Devices Used on the Mississippi River. No. 4. William C. Starrett and Paul G. Barnickol. 41 pp. The Survey, Urbana, Ill., 1955.

Scientific Meetings

Conference on Elementary Particles

An international conference on "Elementary Particles" was held at Pisa, Italy, 12-18 June under the joint auspices of the Italian Physical Society (which was celebrating the first centennial of *Il Nuovo Cimento*, its leading journal) and the International Union of Pure and Applied Physics. Nearly 500 persons attended the conference with the Italians, of course, predominating, although there were large delegations from England (50) and from France (30). The United States delegation was the next largest—about 15—whereas the U.S.S.R. was represented by two physicists, M. Marcov and S. Vernov. The other countries represented at the conference were Austria, Belgium, Chile, Denmark, Germany, Holland, India, Ireland, Israel, Japan, Mexico, Norway, Poland, Sweden, and Switzerland.

The scientific activity of the conference was carried out at three simultaneous sessions: section A on "Heavy unstable particles," section B on "Quantum field theory and meson-nucleon interactions," section C on "Miscellaneous subjects." The first two sections, which essentially constituted the international conference, were conducted in English. The third section, which was obviously intended to take care of the special interests of the Italian Physical Society, was conducted chiefly in Italian. It is perhaps worthy of note that half of the sessions in sections A and B were chaired by Americans: W. B. Fretter (Berkeley), R. E. Marshak (Rochester), G. T. Reynolds (Princeton), M. Schein (Chicago), J. Schwinger (Harvard), R. W. Thompson (Indiana), and E. P. Wigner (Princeton). The remaining sessions were chaired by Italians—E. Amaldi, G. Bernardini, and G. Occhialini—and by C. C. Butler (England), W. Heisenberg (Germany), L. Leprince-Ringuet (France), and E. Schrödinger (Ireland).

Since I attended chiefly the sessions of section A, I shall confine my remarks to this section. Much progress in elementary-particle research was reported since the Fifth Rochester Conference on High Energy Physics was held in January 1955. The chief factors contributing to this progress were (i) further analysis of a

giant cosmic-ray nuclear emulsion stack by a team of European laboratories, in particular the Italian groups at Genoa, Milan, Padua, Rome, and Turin, and by the Bristol, Dublin, and Paris groups; (ii) initiation of elementary-particle research with the bevatron at the University of California by the Berkeley group and by the Bristol and Paris groups in Europe (through the exposure of nuclear emulsions to the K-meson beams from the bevatron); (iii) further cloud-chamber work by many American, British, and French groups.

The second item was particularly significant, since it heralded the beginning of a new era in elementary-particle research. It was clear that—just as in the case of pions—the center of attention would now shift to investigations carried out with artificially produced K mesons and hyperons. And it was precisely because the Bristol and Paris nuclear emulsion laboratories had anticipated this development and had arranged for exposures in the bevatron that the contributions of these two laboratories added so much to the Pisa conference. Indeed, one of the gratifying incidents at the Pisa conference was the vote of thanks adopted by acclamation expressing appreciation to American scientists for making these exposures possible—a resolution that was initially suggested by C. F. Powell of Bristol and presented as a formal motion by M. Conversi, secretary general of the Pisa conference.

Some of the experimental results that were presented in a definitive form for the first time at the Pisa conference were as follows. (i) All K mesons, both charged and neutral, possess the same rest mass within experimental error (965 ± 10 electron masses); this holds true for the neutral θ meson and the charged τ , τ' , K_{23} , K_{21} , K_{32} and the K_{23} mesons. (ii) The lifetimes of all the charged K mesons, within experimental error, are the same—in the neighborhood of 1×10^{-8} second. (iii) Among the charged K mesons, the most abundantly produced are the K_{23} and the K_{21} , ending up with the much rarer τ , τ' , K_{32} and K_{23} mesons. (iv) The cross section for the production of positively charged K mesons (of any variety) is much larger than the cross section for the production of negatively

charged K mesons (of any variety), the production ratio being about 100 to 1 at bevatron energies. (v) The interaction mechanism for positively charged K mesons with nuclear matter is completely different from that for negatively charged K mesons, the former yielding only elastic and inelastic scatterings and disappearances in flight, whereas the latter in addition participate in absorption processes that give rise to several types of hyperons. (vi) Evidence has been found for the production of a cascade-type hyperon in association with two neutral θ mesons.

The summary theoretical talk on the heavy unstable particles was given by M. Gell-Mann (California Institute of Technology). He summarized the remarkable extent to which his phenomenological scheme for the heavy unstable particles—to which A. Pais (U.S.) and K. Nishijima (Japan) have contributed—was supported by all the available experimental data. Gell-Mann's scheme consists in considering four classes of particles—baryons (which include the nucleon and various types of hyperons), mesons (which include the pion and the various types of K mesons), electrons (which include the muon, electron, and neutrino), photons—and three types of interaction—strong, electromagnetic, weak.

Gell-Mann assumes that all strong interactions—which appear to involve only baryons and mesons—are charged independent, so that all strongly interacting particles split up into charged multiplets. However, it is not necessary that the different types of baryons possess the same center of charge as the nucleon. Indeed, the displacement of the center of charge from $e/2$ (the normal nucleon position) for a given type of hyperon, in units of $e/2$, is called the strangeness quantum number S . In a similar fashion, a strangeness quantum number can be defined for mesons; for these particles it is the displacement of the center of charge from 0 (the normal pion position). It is then postulated that both the strong and electromagnetic interactions (assigning $S=0$ to the photon) conserve the total strangeness quantum number, whereas the weak interactions (in particular, the various decay modes for the different classes of K mesons and hyperons) involve a change in the total strangeness quantum number of ± 1 . It is a remarkable fact that all the experimental results cited here and all prior results on the heavy unstable particles can be fitted into this scheme.

Although the Gell-Mann scheme supplies a satisfying coherence to the existing experimental data on the heavy unstable particles, it is admittedly far from being a full-fledged theory. Several reports at the Pisa conference, particularly by B. D'Espagnat and J. Prentki (C.E.R.N.)

and J. Rayski (Poland) attempted to provide a more fundamental theory of elementary particles, but it is perhaps fair to say that a satisfactory theory of elementary particles is still far off.

R. E. MARSHAK

Department of Physics, University of Rochester, Rochester, New York

Meeting Notes

■ A symposium on tuberculosis in infancy and childhood will take place at the National Jewish Hospital in Denver, Colo., 9–12 Nov. Some 150 specialists from France, Great Britain, Greece, Italy, Norway, and Sweden, as well as the Americas, will consider the 21 papers that are scheduled.

France will be represented by Robert Debre of the Hôpital des Enfants Malades and the University of Paris, and Georges Canetti and Noel Rist of the Pasteur Institute. The British participants will be K. Neville Irvine, Oxford Regional Hospital Board; Charles H. Lack, Royal National Orthopedic Hospital, London; and D. A. Mitchison, Postgraduate Medical School, London. Others from Europe will be C. Choremis, Athens University Clinic; Cesare Cocchi, Pediatric Clinic, University of Florence, Italy; Hans Jacob Ustvedt, Ullevaal Hospital and the University of Oslo School of Medicine, Norway; and Lars Strom, Karolinska Sjukhuset, Sweden.

Heading the American participants will be Rene Dubos, of the Rockefeller Institute, New York, whose address will open the symposium.

■ The Nederlandse Natuurkundige Vereniging will hold an International Conference on Nuclear Reactions 1–7 July 1956 in Amsterdam. Among the topics that will be discussed are elastic and inelastic scattering, capture- and photo-reactions, stripping- and pick-up reactions, and fission. Further information may be obtained from the conference committee's secretary, Dr. S. A. Wouthuysen, Zeeman Laboratorium, Pl. Muidergracht 4, Amsterdam (C), Netherlands.

■ A special conference on Nutritional and Metabolic Considerations in Disease will be held on 9 Nov. in Philadelphia, Pa. The conference, which will report on nutrition as it affects cardiovascular diseases, neurology and psychiatry, surgery, and nutritional disturbances, is being sponsored by the commissions on nutrition of the Medical Society of the State of Pennsylvania and the Philadelphia County Medical Society in cooperation with the National Vitamin Foundation.

Among the scientists from various parts of the country who will participate are Norman Jolliffe, City of New York Department of Health; Garfield G. Duncan, Jefferson Medical College; Irvine H. Page, Cleveland Clinic, Cleveland, Ohio; Campbell Moses, University of Pittsburgh; Peter T. Kuo, University of Pennsylvania; Douglas Gordon Campbell, University of California, Berkeley; Ivan F. Bennett, Veterans Administration Hospital, Coatesville, Pa.; I. S. Ravdin, University of Pennsylvania; and William T. Fitts, Jr., University of Pennsylvania. Cochairmen of the meeting are R. S. Goodhart and M. G. Wohl. For information, write to the College of Physicians, 19 S. 22 S., Philadelphia, Pa.

■ More than 80 foreign scientists who have made major contributions in the field of solar energy research will participate in the World Symposium on Applied Solar Energy in Phoenix and Tucson, Ariz., 31 Oct.–4 Nov. At least 1000 scientists, engineers, educators, and industrialists are expected to attend the meeting, which is sponsored by the Association for Applied Solar Energy, Stanford Research Institute, and the University of Arizona. Many conferees will also attend an earlier scientific conference on solar energy at the University of Arizona, Tucson, 31 Oct.–1 Nov.

Participation of foreign scientists has been made possible by financial support from the National Academy of Sciences, the National Science Foundation, the Ford Foundation, the Rockefeller Foundation, the Office of Naval Research, the U.S. Air Force, and UNESCO. The list of visitors from abroad that follows includes the first Soviet physical scientist to visit the United States since World War II.

Algeria: G. A. Betier and M. A. Guillemonat, Commission de l'Energie Solaire; and J. Savornin, University d'Algiers.

Australia: Roger N. Morse and B. W. Wilson, Commonwealth Scientific and Industrial Research Organization; A. H. Willis, New South Wales University of Technology; and J. E. Cummins, Australian Scientific Liaison Office, Washington, D.C.

Austria: Hans Thirring, Vienna.

Belgium: Leon Crespín, Technique de la Chaleur, and M. V. Migotte, Institut d'Astrophysique.

Brazil: Jerome F. Harrington, IBEC Research Institute.

Canada: E. A. Allcutt and M. Hooper, University of Toronto; J. W. Hodgins, Royal Military College of Canada; Fredrik Krug, Montreal; and G. B. Tebo, Ontario Hydro.

Cuba: O. M. Cherenzilind, Economic, Industrial and Technological Guide of Cuba, and Luis Parajon e Hijo.

Egypt: Mostafa Elnesr, Cairo University; Mohammed Mamdouh Fikry, Alexandria; Mostafa Mahmoud Hafez, National Research Council; and Alex Schoenberg, Fouad I University.

England: E. C. Bullard, National Physical Laboratory; R. Fitzmaurice, Guildford; the Earl of Halsbury, National Research Development Corp.; Harold Heywood, Imperial College of Science and Technology; Willis Jackson, Metropolitan Vickers, Ltd.; J. K. Page, Department of Scientific and Industrial Research, Building Research Station; W. H. Pearsall, University College, London; N. W. Pirie, Rothamsted Experimental Station; Jack Pritchard, London; and C. P. Wittingham, Botany School, Cambridge.

France: Paul Jean Bergeron and Jean Maurice Horace Guerin of the Scientific Action Committee for National Defense of France; P. Chouard, Sorbonne; R. Donn, French Embassy, Washington, D.C.; Pierre Donzelot, French universities representative, New York; G. Dupouy, Centre National de la Recherche Scientifique; Marcel Floret, Electricité de France; Marc Foex, Laboratoire de l'Energie Solaire, Citadelle de Montlouis; François LeBras, Bureau d'Organisation des Ensembles Industriels Africains, Paris; Werner F. Moller, UNESCO, Paris; Felix Trombe, Laboratoire de l'Energie Solaire, Ecole Normale Supérieure de Chimie; and P. L. Schereschewsky, French Office of Power Stations, Washington, D.C.

French West Africa: Henri Masson, Institut des Hautes Etudes de Dakar.

Germany: Fritz Gummert, Maria Meffert, and A. Stratmann, Kohlenstoffbiologische, Essen; Richard Harder, Botanisches Institut, University of Göttingen; Hans Lagoni, Physical Institute for the Federal Station on Milk Products, Kiel; Hermann F. Mueller, Gesellschaft für Praktische Energie, Kunde, Technische Hochschule; C. P. Tingwaldt, Physikalisch-Technische Bundesanstalt; Friedrich Tonne, Institut für Tageslicht Technik; and H. T. Witt, Physikalisch-Chemisches Institut der Universität Marburg.

India: M. L. Khanna, K. S. Krishnan, and K. N. Mathur of the National Physical Laboratory of India.

Israel: D. Ashbel, Hebrew University; Rudolph Block, Dead Sea Works, Ltd.; Nathan Robinson, Solar Radiation Laboratory, Israel Institute of Technology; H. Tabor, National Physical Laboratory; and Louis F. Yissar, Holon.

Italy: Giuseppe R. Badoni, Consigliere Delegato; Gino Bozza, and Mario Dornig, Politecnico de Milano; Franco Castelli, Steam Power Division, Società Edison; Luigi D'Amelio, Naples University; F. Filippi, Centro Nazionale Meccanico Agricolo, Consiglio Nazionale delle Ri-



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Lebanon: Adnan Tarcisi, Yemen Office for Syria and Lebanon.

Malaya: Gerald T. Ward, physics department, University of Malaya.

Mexico: Nabor Carillo, Universidad Nacional de Mexico; Michel Fournier de Alba, Instituto de Geofisica, Torre de Ciencia, Universidad Nacional Autonoma de Mexico; J. K. Jennings, the Mexican Light & Power, Ltd.; and David Matson, Compania Impulsora de Empresas Electricas S.A.; George S. McLaughlin, Cuadrante de San Francisco 48.

Morocco: R. Ambroggi, Centre des Etudes Hydrogeologiques, Direction de la Production, Industrielle et des mines.

Netherlands: L. N. M. Duysens, Fysisch Laboratorium der Rijks Universiteit, Utrecht, and B. Kok and E. C. Wassink of the Laboratorium Voor Plantenphysiologisch, Onderzoek der Landbouwwogeschool, Wageningen.

Netherlands West Indies: P. C. Henriques, Curaçao.

New Zealand: C. J. Banwell, Dominion Physical Laboratory.

Thailand: Ravi Pavelai and Sukum Sritanyarantana, Chulalongkorn University.

Union of South Africa: Arthur E. H. Bleksley, University of Witwatersrand, and Austin Whillier, South African Council for Scientific and Industrial Research.

U.S.S.R.: V. A. Baum, deputy director, G. M. Krzhizhanov Power Institute, Academy of Sciences of the U.S.S.R.

Forthcoming Events

November

28-1. White House Conf. on Education, Washington, D.C. (C. Pace, Director; Comm. for White House Conf. on Education; South Health, Education and Welfare Bldg.; Washington 25.)

29-2. American Medical Assoc., clinical, Boston, Mass. (G. F. Lull, AMA, 535 N. Dearborn St., Chicago 10, Ill.)

29-2. Entomological Soc. of America, Cincinnati, Ohio. (R. H. Nelson, 1530 P St., NW, Washington 5.)

December

2. American Alpine Club, annual, New York, N.Y. (J. C. Oberlin, 900 Leader Bldg., Cleveland 14, Ohio.)

2-3. American Federation for Clinical Research, Eastern, Philadelphia, Pa. (C. R. Shuman, Temple Univ. Hospital, Broad and Ontario Sts., Philadelphia 40, Pa.)

2-3. Oklahoma Acad. of Science, Norman. (D. E. Howell, Dept. of Entomology, Oklahoma A. & M. College, Stillwater.)

2-4. American Psychoanalytic Assoc., New York, N.Y. (J. N. McVeigh, 36 W. 44 St., New York 36.)

4. American Acad. of Dental Medicine, 10th mid-annual, New York, N.Y. (G. J. Witkin, 45 South Broadway, Yonkers 2, N.Y.)

8-10. Concept of Development, Minneapolis, Minn. (D. B. Harris, Inst. of Child Welfare, Univ. of Minnesota, Minneapolis 14.)

8-10. Florida Acad. of Sciences, Miami. (R. A. Edwards, Geology Dept., Univ. of Florida, Gainesville.)

9-10. Assoc. for Research in Nervous and Mental Disease, 35th annual, New York, N.Y. (C. C. Hare, 710 W. 168 St., New York 32.)

9-10. Texas Acad. of Science, annual, Waco. (G. P. Parker, P.O. Box 7488, College Station, Texas.)



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10-16. Nuclear Cong. and Atomic Exposition, Cleveland, Ohio. (A. F. Denham, 931 Book Bldg., Detroit 26, Mich.)

10-16. Radiological Soc. of North America, Inc., Chicago, Ill. (D. S. Childs, Sr., 713 East Genesee St., Syracuse 2, N.Y.)

11-14. American Soc. of Agricultural Engineers, Chicago, Ill. (F. B. Lanham, ASAE, St. Joseph, Mich.)

11-14. American Soc. of Refrigerating Engineers, New York, N.Y. (R. C. Cross, ASRE, 234 Fifth Ave., New York 1.)

14. Operations Research Symposium, Philadelphia, Pa. (R. V. D. Campbell, Operations Research Symposium Registration, Burroughs Research Center, Paoli, Pa.)

15-17. Acoustical Soc. of America, Providence, R.I. (W. Waterfall, ASA, 57 E. 55 St., New York 22.)

15-17. International Union of Scientific Radio, U.S. national, Gainesville, Fla. (J. P. Hagen, Code 7100, URSI, Naval Research Lab., Washington 25.)

16-21. Interamerican Cong. of Psychology, 3rd, Austin, Tex. (W. Holtzman, Univ. of Texas, Austin.)

26-29. Biometric Soc., Eastern N. American Region, New York, N.Y. (A. M. Dutton, Box 287, Station 3, Rochester 20, N.Y.)

26-31. American Assoc. for the Advancement of Science, Atlanta, Ga. (R.

L. Taylor, AAAS, 1025 Connecticut Ave., NW, Washington 6.)

27-29. American Mathematical Soc., 62nd annual, Houston, Tex. (J. H. Curtiss, AMS, 80 Waterman St., Providence 6, R.I.)

27-29. Archaeological Inst. of America, Chicago, Ill. (C. Boulter, 608, Univ. of Cincinnati Library, Cincinnati 21, Ohio.)

27-29. Assoc. for Symbolic Logic, Rochester, N.Y. (J. Barlaz, Rutgers Univ., New Brunswick, N.J.)

27-29. Linguistic Soc. of America, Chicago, Ill. (A. A. Hill, 1719 Massachusetts Ave., NW, Washington 6.)

27-29. Western Soc. of Naturalists, Davis, Calif. (D. Davenport, Univ. of California, Santa Barbara.)

27-30. American Statistical Assoc., New York, N.Y. (E. M. Bisgyer, 1757 K St., NW, Washington 6.)

27-30. Inst. of Mathematical Statistics, New York, N.Y. (K. J. Arnold, Dept. of Mathematics, Michigan State Univ., East Lansing.)

27-1. Phi Delta Kappa, 50th anniversary, Bloomington, Ind. (J. C. Whinnery, 324 N. Greenwood Ave., Montebello, Calif.)

28-29. Northwest Scientific Assoc., Spokane, Wash. (F. J. Schadegg, Eastern Washington College of Education, Cheney.)

28-30. American Economic Assoc., New York, N.Y. (J. W. Bell, Northwestern Univ., Evanston, Ill.)

28-30. American Historical Assoc., Washington, D.C. (B. C. Shafer, Study Room 274, Library of Congress Annex, Washington 25.)

28-30. American Philological Assoc., Chicago, Ill. (J. P. MacKendrick, Bascom Hall, Univ. of Wisconsin, Madison 6.)

28-30. Low Temperature Physics and Chemistry, Baton Rouge, La. (J. G. Daunt, Dept. of Physics, Ohio State Univ., Columbus 10.)

28-30. American Philosophical Assoc., Eastern Div., Boston, Mass. (W. H. Hay, Dept. of Philosophy, Univ. of Wisconsin, Madison.)

28-30. American Physical Soc., winter meeting, Los Angeles, Calif. (K. K. Darrow, Columbia Univ., New York 27.)

28-30. Econometric Soc., New York, N.Y. (R. Ruggles, Box 1264, Yale Station, Yale Univ., New Haven, Conn.)

29. Metric Assoc., Inc., annual, Washington, D.C. (V. G. Shinkle, 1916 Eye St., NW, Washington 6.)

29-30. American Folklore Soc., Washington, D.C. (M. Leach, Bennett Hall, University of Pennsylvania, Philadelphia 4.)

29-30. History of Science Soc., Washington, D.C. (T. S. Kuhn, 74 Buckingham St., Cambridge 38, Mass.)

30. Mathematical Assoc. of America, 39th annual, Houston, Tex. (H. M. Gehman, University of Buffalo, Buffalo 14, N.Y.)

(See 21 Oct. issue for comprehensive list)

Equipment News

■ **STEREOSCOPIC CAMERA** that is capable of taking three-dimensional photographs in color or black and white has been announced. Invented by David Donaldson of the Howe Laboratory of Ophthalmology, Harvard Medical School, the camera is designed to eliminate the optical distortions inherent in ordinary stereo equipment. Bellows extension, inter-lens distance, and parallax correction scales are calibrated according to magnification. Once the appropriate magnification or minification has been selected, all scales are set to this number before the camera is focused. A variable intensity light source (electronic strobe flash) of short duration, powered by a separate amplifier, is also calibrated according to magnification. The instrument is designed so that the user can duplicate exposure, magnification, and stereoscopic effect. A built-in stop prevents double exposures; the lens openings remain constant at a small aperture. (Perkin-Elmer Corp., Dept. Sci., Norwalk, Conn.)

■ **ADSORPTION ALUMINAS** of analytic grade have been processed and standardized for chromatographic use. Acid, basic, and neutral materials of activity grade I (Brockmann) are currently available. (Bio-Rad Laboratories; Dept. Sci., 800 Delaware St., Berkeley, Calif.)

■ **RESOLUTION TEST PATTERNS** on glass, designed for use in determining the resolving power of photographic lenses and other equipment and in evaluating the relative performance of lenses, are available singly or in quantity. Bulletin 8000. (W. and L. E. Gurley, Industrial Div., Dept. Sci., Troy, N.Y.)

■ **METALLURGICAL MICROSCOPE** has an oversized focusing stage with interchangeable stage plates, a fine-adjustment focusing knob located at table level, and an accessory lens for concentrating external illumination into the microscope for photomicrographic work. A new vertical illuminator and triple-revolving objective turret that makes possible the study of opaque or semiopaque specimens in brightfield, darkfield, or polarized light is also available. Literature D-1053. (Bausch and Lomb Optical Co., Dept. Sci., 635 St. Paul St., Rochester, N.Y.)

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Program content

1. The four-session symposium, "Atomic Energy and Agriculture," arranged by ORINS.
2. The three-session program of the International Geophysical Year.
3. An AAAS-sponsored "Congress" on the shortage of young scientists and science teachers.
4. Programs of the 17 AAAS sections (symposia and contributed papers).
5. Programs of the more than 60 participating societies.
6. The Special Sessions: AAAS, Academy Conference, Conference on Scientific Editorial Problems, National Geographic Society, Phi Beta Kappa, RESA, Sigma Xi.
7. Details of the Municipal Auditorium—center of the Meeting—and hotels and campuses.
8. Titles of the latest foreign and domestic scientific films to be shown in the AAAS Science Theatre.
9. Exhibitors in the 1955 Annual Exposition of Science and Industry and descriptions of their exhibits.

Directory content

1. AAAS officers, staff, committees for 1955.
2. Complete roll of AAAS presidents and their fields.
3. The more than 265 affiliated organizations.
4. Historical sketch and organization of the Association; the Constitution and Bylaws.
5. Publications of the Association.
6. AAAS Awards and Grants—including all past winners.
7. Membership figures by sections.
8. Section committees (Council members) in detail.
9. Local committees.
10. Future Meetings of the AAAS through 1962.
11. New and current activities of the AAAS.

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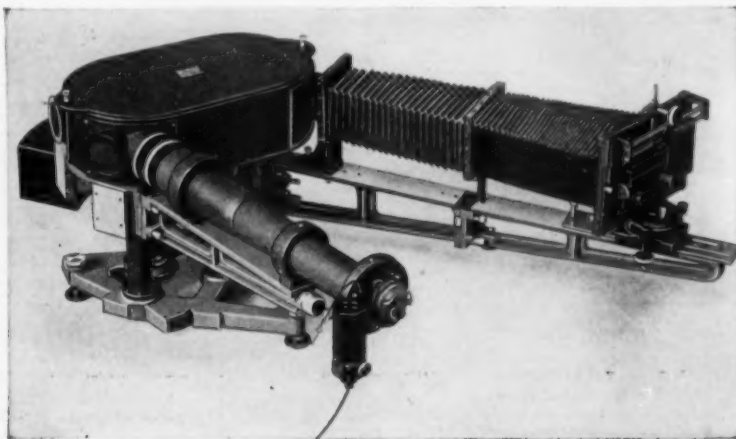
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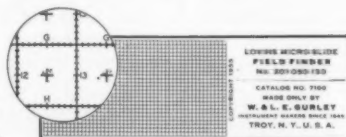
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